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THE

PHILADELPHIA

MEDICAL MUSEUM,

CONDUCTED

BY

*JOHN REDMAN COXE, M. D.*

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VOL. IV.

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Philadelphia :

PRINTED BY BARTRAM AND REYNOLDS;  
FOR THOMAS DOBSON, AT THE STONE HOUSE No. 41, SOUTH SECOND  
STREET,—AND FOR SALE BY THE BOOKSELLERS IN DIFFERENT  
PARTS OF THE UNITED STATES.

.....

1808.

## *District of Pennsylvania, to wit:*

BE IT REMEMBERED, that on the twenty-sixth day of December, in the thirty-second year of the Independence of the United States of America, A. D. 1807, Thomas Dobson, of the said District has deposited in this office the Title of a Book the right whereof he claims as proprietor in the words following, to wit:

*"The Philadelphia Medical Museum, conducted by John Redman  
Coxe, M. D. Vol. IV."*

In conformity to the Act of the Congress of the United States, intituled "An Act for the encouragement of learning, by securing the copies of Maps, Charts, and Books to the authors and proprietors of such copies during the times therein mentioned," And also to the Act entitled "An act supplementary to the act entitled "An act for the encouragement of learning by securing the copies of Maps, Charts, and Books to the authors and proprietors of such copies during the times therein mentioned," and extending the benefits thereof to the Arts of designing, engraving, and etching, historical and other prints,"

D. CALDWELL,

*Clerk of the District of Pennsylvania.*

THE very favourable reception which the *Philadelphia Medical Museum* has met with from the Physicians of America during a period of nearly four years, cannot but be highly flattering to the Editor; and while he announces the completion of a fourth volume of the work, the hopes of its having proved beneficial to the extension of medical information, must necessarily excite his warmest wishes and exertions for its continuance. In returning his sincere thanks to the patrons of the Museum, and especially to those gentlemen whose valuable communications have been so liberally bestowed; he most respectfully solicits a continuance of those favours, by which it has reached its present standing, and trusts in their co-operation to render it of increasing utility.

It will be perceived from the present volume, that another periodical publication is about to commence in Baltimore; and from the industry and application of its Editor, there can be but little reason to doubt of its success. When we consider how few years have elapsed since the first work of this kind began its career in this country, we cannot but regard it as a highly favourable pledge of the extension of Medical Science, that a further increase of this species of literature, should be deemed necessary; and we cherish the hope, that such favourable auspices will long continue to enlighten the extensive regions of America.

*Philadelphia,*  
*January 1st. 1808.*



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# MEDICAL MUSEUM.

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VOL. IV.....NO. I.

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*New Observations on Syphilis, tending to settle the Disputes about its Importation, by proving that it is a Disease of the Human Race, that has, and will always exist among the several Nations of the Globe. By J. C. ROUSSEAU, M. D. Read before the PHILADELPHIA MEDICAL LYCEUM, January 21<sup>st</sup>. 1807.*

**M**ORE time has been spent to find out the origin, than to ascertain the cause of the venereal disease.

As remote as the eleventh century, says Becker, "we find in some English papers, that mention is made of a disease called *burning*, and a law forbidding any keeper of bawdy houses, to keep women labouring under it." The general opinion, however, seems to be fixed upon a later date, and warm controversies have taken place at different periods, and among different nations respecting its introduction.

To ascertain with exactness, who deserves the blame, is a matter of no consequence, and it would require an immense loss of time without any benefit. Although the denomination of morbus Gallicus, appears to have irrevocably fixed upon it a French origin, the French do not hesitate to devolve the merit

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of the claim to the Neapolitans and Spaniards ; a further proof of uncertainty.

Had the first discoverer of this scourge of the human race, like a Jenner, been able to turn its poison, against a more formidable disease ; the nation that now pays the compliment to its neighbour, would vindicate to itself the honour of the introduction.

But instead of rejecting the blame upon one another, had all nations, *à principio*, considered *Lues Venerea* as a disease incident to the human race, like pleurisy, dysentery and others ; we would have probably been, long ago, in possession of the means of curing it as easily, nay, perhaps of preventing it as effectually, as we do the itch, and many other diseases, contagious by immediate application of their virus.

Indeed the variety of forms, under which this disease makes its appearance, ought to have induced medical men to look for different sources ; for in many instances we find it a mere local disease, and remaining so for years without affecting the constitution ; in some others, its ravages go on with an extreme rapidity, and in a few cases, extremely rare indeed, its poison is possessed of such malignity, as to defy the power of all remedies.

From such irregularity arises the difficulty of ascertaining the nature of this disease ; especially if we take into consideration, that its different forms, require modes of cure, very materially different.

It will be suggested, and I am ready to admit, that the violence of its symptoms is often the result of the constitution, but their extreme irregularity can never induce me to suppose that this disease is one and the same in every case ; and such, as is believed to have been imported among us.

After all, should we admit its introduction to be a matter of fact; as it gives no information of its natural origin, and the one who carried it must have contracted it in some way, we must have recourse to another source.

Wishing to extend the sphere of their knowledge far beyond their reach, men have at all times attributed the evils surrounding them, either to occult powers, or to remote causes, leaving an enemy at home, to go in pursuit of another that never existed.

Having erred like others, through the vacuum of theories, without any benefit to myself, and to the detriment of others; fatigued of the burthen of false notions inculcated in the schools; I then began to look about me, and determined to spy nature in its operations. But apprehensive of exchanging errors for errors, I tested every thing by an observation scrutinized by experiments.

Yet fully confident of the difficulty that is always encountered, in attempting to elucidate old prejudices and errors, it was with extreme diffidence, although supported by a long observation of facts, established by a series of experiments, now firmly corroborated by others, that I undertook, several years ago, to refute the system of cutaneous absorption, but I must confess, that it is with reluctance I now undertake to present new opinions on the venereal disease.

I am too sensible of the obstacles that I am to meet with, to enter into such an inquiry, without having previously gathered a quantity of facts, sufficient to enable me to support the opinions that I am to advance.

From the whole, I am now convinced that *lues venerea*, A PRINCIPIO, is no more than a disease generated by an acrimony

of the fluids secreted by the organs of generation, without any previous infection; like the lues bovina in cows, hydrophobia in dogs, ophthalmia, variola and a number of cutaneous eruptions, in the human race, all contagious by immediate application or introduction of their virus.

That the disease will assume different forms, according to the different parts to which it is applied, the constitution of the individual receiving it, the vitiated quality of the fluids secreted by the infecter, and the affinity between the fluids of the infecter and infected.

Relating experiments, to support these opinions, would submit their author to public censure, although they would perhaps insure to him the unbounded gratitude, and unreversed thanks of every private individual, in the secrecy of his heart.

Prejudices, delicacy, morality; I have too much respect for you, to go too far beyond your limits; but should your consideration prevent me from blunting the thorn that grows under the rose?

Since the shame attached to the disease, now under consideration, no more than the terror of all the evils and ravages produced by it, have as yet been able to put a stop to its continuation; it is now time, I think, to endeavour to find out the means of preventing its production, as far as it may lie in our power, and this, you will all admit, cannot be done, without a perfect knowledge of the cause and nature of its virus.

Although the opinion of this disease, being produced by a vitiated state of the fluids secreted by the organs of generation, is not altogether a new one; it has only been delivered as a mere opinion, without being supported by a single fact, that could give it a degree of probability.

Several years spent in observing and curing this disease, have enabled me to support this opinion by a collection of facts, often put to the test of experiments. But as a short essay does not admit of a long series of observations, I shall confine myself to a selection of some of the most striking cases.

The gonorrhœa infantum, is undoubtedly produced, without any intercourse of sexes, in both males and females: and I have seen this disease run so high, as to be necessitated to perform the operation of the phymosis, upon children that never had any knowledge of women.

I have seen several decided cases of gonorrhœa, which lasted for a considerable time in male children, of five, seven and upwards. I have seen it in females in almost all periods of childhood, and in many instances it produced excoriations, which appeared very much like chancres: how such infants might have infected others, I am unable to tell from the impropriety of trying the experiment; but I form no kind of doubt, from having seen several play-mates affected in the same way, that they had contracted it from one another by manual admission.

The multifarious evacuations per vaginam, all comprehended under the improper denomination of fluor albus, exhibit such a variety of colours and qualities, as to make us suppose that they all have a different character: they often possess such a degree of acrimony, as to ulcerate the skin, not only of the labia, but of the thighs, when, from appearance, no such corrosive quality could ever be suspected; and in numerous instances, these ulcers resemble so much chancre, that I have seen patients, under those circumstances, undergo a mercurial course by the advice of a physician.

A young girl of 19, was under the care of a physician for a pretended gonorrhœa; the discharge was so acrid, as to cor-

rode and produce small ulcers, on the nymphæ and thighs; three months having been spent in taking medicines and using local applications, without any alteration of the disease, she determined to let it have its own course; but a while after, some of her friends having persuaded her to take my advice, she consulted me. On examination of the parts, I found nothing that could raise the least suspicion of a gonorrhœa. I therefore gave her as my opinion, that it proceeded from the uterus, and after having consented to an examination, by means of a speculum contrived for that purpose, I was convinced of the truth.

Injections were then directed into the cavity of the uterus, by appropriated instruments, which relieved her for a while, but never operated a cure. The discharge has now been continuing better than two years, without receiving any benefit from the several medicines she has taken. She has been charged now and then of having communicated infection, although in a hundred instances, no complaint at all, has been made against her.\*

In numerous instances, I have seen both chancres and gonorrhœa, produced from the intercourse with a woman that had no sign of either.† I have watched and seized every opportunity to ascertain this fact, and a strict examination has satisfied me of its exactness. In one case, especially, I must observe, that the chancres produced, were treated by local appli-

\* She has lately contracted a gonorrhœa and got well of it, without producing any alteration on the former disease.

† I have now under my care, a young girl of about seventeen, who is a striking proof of this: she has injured a dozen of young men, some with a clap, and others with chancres, and has neither of these diseases. The inspection offers a secretion inside of the vagina, as thick and white as lard, very much resembling the sebacious matter found upon new-born infants.



cations, and some internal medicines for several months, without shewing any disposition to heal; until mercury was resorted to, and effected a cure.

At other times, I have seen women with excoriations on the os externum, produced by repeated exercise of the parts, acquire such a degree of acrimony, as to communicate a gonorrhœa. These women had no gonorrhœa, for I ascertained it by repeated examinations, it being as easy to discover the existence of this disease in women, as it is men.

A virulent disease, is without doubt, oftener generated and communicated by women of venereal profession, than by others; but, I take it to be the case, that in numbers of instances, a disease of an inferior virulence, has been communicated by extremely virtuous women.

A gentleman of respectability, married to a virtuous and extremely regular lady, consulted me at three different periods, with no small concern.

The glans penis was blistered all over and very much inflamed,\* he did not know what to think of it, for he declared positively that he had no connexion with any other woman, but his wife. I told him of the possibility of virtuous women injuring their husbands, related to him several instances that had fallen under my observation, directed the mode of cure for both, assuring him that I did not think, nor had ever seen, that in women of regular life, the humours could acquire such a degree of virulence as was necessary to produce a disease that required the use of mercury, especially, where immediate means were resorted to. In a short time he got well, and I did not see him till about three months after, when he return-

\* This disease is called by B. Bell and others, a particular kind of chancre.

ed to me in the same condition. I repeated my former observations, and directed the same mode of cure.

I had some time after, the displeasure to see him call for a third time, but I was so firm in my opinion, that I begged of him not to alter it: advised him in strong terms to inculcate to his wife the necessity of using for a long time the remedies that I had prescribed, and particularly the *vaginæ-luvium*; they had both the satisfaction of getting well, without any remains of suspicion against each other; and without undergoing the fatigue and inconvenience of a mercurial course.

In two other cases, (but I will relate particularly one, for no kind of doubt can be raised respecting its veracity,) a kind of sore, extremely painful, was produced upon the frenum, by no other connection than with a regular and virtuous wife, beyond all kind of suspicion. It was in the case of a gentleman of the medical profession. He informed me that about a year before, he had been troubled with a small sore, dividing transversely the frenum, that it was extremely painful and lasted a considerable time without increasing, and resisting all kind of remedies that he had been able to contrive. That he was then troubled with the same for a second time, and unable to account for such a disease, having never made a breach of the matrimonial duties; nor had he ever contracted any venereal complaints in his life.

I told him that I had seen a case of the same nature, which was very troublesome, and after using a diversity of remedies, I had applied the *unguentum ex hydrargyro nitrato*, which, in a few days completed the cure: he used it, and in a few days was well, and with a free use of ablutions, his wife had the satisfaction to see him enjoy a *rose sans épine*.

I shall now be asked, were the diseases above related of a contagious nature? Here again our steps are prevented from going

any further into the inquiry, not by the impossibility, 'but, by the impropriety of trespassing over the limits that I have promised to respect. But if analogy can be resorted to, I will be permitted to relate a case, in which a disease of the most trifling appearance, far below the cases above mentioned, has transmitted a disease of the most alarming nature.

A young man, in a spell of merry enjoyment, having forgotten that he had left at home his female companion, indulged himself in a taste of variety: come to himself again, and hardly recollecting his dream, he returned to his wife without any apprehension of being in a situation capable of injuring her. A day or two after, beginning to feel an uneasy sensation upon the glans penis, he found on examination that he had three little sores as big as the head of a pin. He immediately applied some blue stone, as he called it, (sulphate of copper) and in the course of three days, was as well as ever; shortly after went to sea, leaving his wife unapprised of the impending evil.

A few days after his departure, painful sensations in her inguinal regions, were soon followed by the appearance of swellings. She informed her sister, who having consulted me, persuaded her to relate to me, herself, the situation she was in. Her delicacy was the cause of some delay, but her disease becoming more alarming, turned all considerations into necessity. She then discovered two large buboes, one of which, in spite of all remedies, went on with such a rapidity, that in a few days I was obliged to open it. The disease did not stop there; the skin over the bubo ulcerated, and in a few months, there was a loss of substance better than three inches in length and two in breadth: and more than six months were spent, before I could see her perfectly cured. Three years have now nearly elapsed, and the husband has never exhibited any symptoms

of syphilitic affection, and their offspring is perfectly sound and healthy.

In some constitutions chancres will heal with cleanliness alone; in others, the use of mercurial preparations often aggravates the disease.

These few cases, selected from an immense collection of observations, will be, I am convinced, sufficient to shew, that the vague appellation of venereal disease, does not fix our attention upon a particular point, easy at all times to be ascertained, making this disease an unit by its character and symptoms, like the small pox, vaccina, hydrophobia, &c.; for if it is a disease imported among us, why do we not see it in every case, with the same symptoms and the same periods in its course.

But further, what shall we say of a woman having several chancres, and not communicating the disease? It is a truth that I am well convinced of, that this disease can never be communicated, when caught by infection; unless the matter of chancres, come into an immediate contact. This has been confirmed so often by experience, that it is a truth as well established with me, as the existence of the light of the sun at noon.

It is certainly more consistent with reason, to consider this disease as proceeding from a depraved state of the secretions of the genital organs, especially when they are stimulated by repeated irritation.

We see often, ophthalmia produced by an irritation applied to the eyes, and the fluid secreted in that disease, will, by its application, produce the same complaint in another individual, as I have witnessed Does not this resemble a gonorrhœa?

What is a chancre, but the place where the disease was inoculated? But this chancre does not transmit to other parts of the constitution the power of infection, unless the whole has been vitiated by it; and yet no fact can prove, that without the existence of an ulcer upon the parts coming into a contact, the same disease could ever be transmitted.

The mad dog labouring under hydrophobia, does not exhibit any ulcer; shall we then suppose that he cannot diffuse his poison, as well as the one that carries the bite that inoculated the disease?

How is the small pox generated in the individual that propagates it?

How is the grease produced in the foot of the horse? and how can it, if we believe the history of the vaccine, by its application to a particular organ of the cow, produce a disease, which transmitted into the human frame, becomes an antidote against one of the most formidable diseases that attack the human race?

These are questions as yet unanswerable. A close and judicious observation of the phenomena of life, would have far more benefited the science of medicine, than all the theories which, like the controversies upon the golden tooth, often labour very hard to explain, phenomena and diseases that never existed.

*An Account of Pulmonary Consumption cured by a Salivation. In a Letter from MR. THOMAS WHITTAKER, Teacher of Mathematics in the Academy of York-Town, to DR. MAXWELL M'DOWEL of the same Place.*

*York-Town, December 26th, 1806.*

SIR,

**A** GREEABLY to your request, I will now endeavour to give you a brief statement of the nature of my complaint, such as it appeared to me, at the time I found myself under the necessity of submitting to a *mercurial course* under your direction; together with such circumstances relating to the general state of my health, previous to that time, as my recollection can at present command. Before I proceed, however, it may be proper to observe, that as I am totally unacquainted with the languages from which your professional technics are said to be derived, I must of necessity be very sparing in the use of them, but will endeavour to supply the defect with plain English.

Previous to the summer of 1798, I had enjoyed an uninterrupted state of good health, and possessed a constitution apparently sound and unimpaired. At that time I experienced a slight attack of a fever, which I believe my physician considered of the bilious kind; and which readily yielded to the mode of treatment he adopted. In the summer following I suffered a similar attack; and in the summer of 1800 I first felt what it was to be afflicted with that species of intermittent called an ague. This however, after a few paroxysms, was removed by a plentiful use of the Peruvian bark; and its return, through the fall and winter following, prevented by the same means. In the spring of 1801, I again suffered a severe attack of fever, which eventually settled into a lingering intermittent, with which I had to contend during the greatest part of the

following summer. This however, as the others had done, yielded to the bark ; and by a liberal use of that valuable medicine, with a few other tonics, through the fall and winter of 1801, I was so far restored as to enjoy a tolerable state of health through the spring, summer and fall of 1802.

Perhaps it may be worthy of remark, that from 1797 till the beginning of 1802, I had resided in the neighbourhood of the Susquehanna river ; in places too where the inhabitants were generally more afflicted with intermittents, than those of places more remote from the river. At the latter period I had removed to a distance from the river, where I resided one year, to wit, 1802.

Perhaps it may have been owing to my being at this time out of the atmospheric influence of the river, and living in a place whose climate was more congenial to my constitution, that I enjoyed an exemption from the ravages of disease at this time, as related above. Be that as it may, the prospect of a more eligible situation again tempted me to settle on the bank of the Susquehanna, though not where I had formerly lived. Here I again became subject to occasional attacks of the intermittent, in various forms, and continued to be so till the fall of 1804, when the attacks became so frequent and violent, that I was obliged to relinquish my occupation, and confine myself to my room for upwards of two months.

From this time I began to perceive myself labouring under a sort of constitutional debility, and although the approach of winter, together with the application of my usual remedy, the bark, succeeded in checking the progress of my disorder, at least so far, as to enable me to attend to my business ; yet my health was at this time become very precarious, and my constitution manifestly impaired.

Under these circumstances, I removed to this place in September 1805; and about five weeks after was again seized with an intermittent, which, after assuming various forms, settled into that kind of ague, which, I believe you call a quartan, renewing its paroxysms every fourth day. Then it was, that the usual remedies proved ineffectual. The potent virtues of the bark itself seemed to fail. And although I never placed much confidence in the prescriptions of old women and quacks, yet I was sometimes induced to try their efficacy. All my endeavours however, to remove the complaint proved ineffectual. I sometimes indeed, succeeded in procuring an interval of two or three weeks, by means of the bark; but, to such a degree of debility was my frame reduced by this time, that, whenever a change took place in the atmosphere, I caught fresh cold; the inevitable consequence of which was, a return of my old complaint.

Thus did I continue, the afflicted victim of a wasting disease, from the beginning of October 1805, till the 20th of May last, a period of between seven and eight months, when I found my complaint evidently assuming a more dangerous complexion.

With a frame exceedingly emaciated, my lungs strongly affected, an uneasiness and stricture on my breast, accompanied with almost incessant coughing; I was led to fear that a consumption was about to close the scene. I had also, as I believe I have before this observed to you, some apprehension of a predisposition to that disorder, from the nature of my constitution, habit of body and mode of life: and knowing that some of my ancestors had been carried off by the same disorder, I had even an apprehension that I might have inherited such a predisposition from them.



Such had been the progress of my disorder previous to, and such was my situation at the time I called upon you, on the 20th of May last, and by your advice, relinquished all my former modes of treatment to try the effect of *mercury*. The result, as you know, proved favourable. No sooner was my system sufficiently charged with the mercury, to produce salivation, than the disorders of my breast and lungs vanished. I was confined to my room about four weeks, and in less than six, was able to attend to my business.

From that time to the present, I have enjoyed a perfect exemption from every species of disease: and my breast and lungs are, I think, as sound at this day as ever they were.

From the foregoing statement, you will readily perceive that my opinion is decidedly in favour of the mercurial treatment, in all cases similar to my own. Indeed my opinion is, that where a patient finds himself labouring under a chronic complaint, he would find his account in rejecting the use of inferior medicines, which too often only tamper with his disorder, and adopt the use of the mercury; especially if recommended by his physician. I am clearly convinced, that the use of this medicine, would produce the most happy effects in many complaints affecting the breast and lungs.

I am, Sir,

Your Obedient Servant,

THOMAS WHITTAKER.

*Account of the Typhus Fever, and its Treatment. By DR.  
ELISHA NORTH.*

GOSHEN, *Connecticut*, Jan. 13th, 1807.

SIR,

THE typhus fever for two years past has been very prevalent, in various parts of this country, and attended in many places, with great mortality. Having been successful in the treatment of it, I have been induced to communicate to you, the following history; hoping that it may be useful.

The first person who had the fever in this town, in October 1805, caught it on a journey to the westward. He died on the 21st day of his illness. His fever was suffered to progress, with the assistance of very little medical treatment, until death closed the scene. I saw him, as consulting physician, two or three times; but was prevented from doing what I thought was proper, by the ignorance and prejudice of his friends. I therefore left him.

From this man sixteen ultimately had the fever; one taking it after another. It was confined principally to three families; relatives to the deceased: most of whom had it.

The following were the symptoms. They were pretty uniformly drooping a day or two or more, previous to their being taken down. Their eyes appeared dull; they were inactive; but made little complaint.

They were then taken with the usual symptoms of fever: such as agues, head-ache, back-ache, nausea, want of appetite, thirst, furred tongue: the pulse was weak and quick. Some

however, had considerable fulness of pulse, and symptoms of considerable re-action.

As the disorder advanced, they had great prostration of strength, low delirium, dark-coloured and fœtid diarrhœa, tremor, watchfulness, subsultus tendinum, stupor. They had two exacerbations of fever daily. These symptoms went on increasing, or remained stationary until the 14th day; when a crisis took place. In some, but not in all, a sweat broke out about this period.

After this, the symptoms of amendment gradually became evident. In a day or two, the appetite began to return; the tongue became clean and moist: the pulse more regular, and sleep less disturbed. In the progress of amendment, the appetite became strong, and the other functions of health gradually returned.

The following was my mode of treatment. The first medicine I exhibited, was a full dose of tartarized antimony. I then gave a cathartic of calomel and jalap, or calomel and rhubarb; unless the emetic happened to operate by stool. For the first four or five days, after having cleansed the first passages, I depended principally upon diluent drinks *acidulated*, and *yeast* given once in three hours. The dose of yeast, was from one to two table-spoonfuls.

After a few days, I began to support the strength of the patient, by giving Peruvian bark and Madeira wine. *In the exhibition of these two medicines, I was particularly careful not to use them so freely as to over stimulate the system.* For an adult, the following was the general direction.

Take, Cold infusion of cinchona ʒij.

Tinct. cinchon. comp. ʒj.

Tinct. opii gtt. v.

M. for a dose;

This was taken once in six hours. Madeira wine,  $\bar{z}$ ij, to be taken once in six hours, alternating it with the bark; so that one may be taken once in three hours.

This quantity was varied according to the age and sex of the patient. In some I lessened, in others I increased their usual quantity of medicines, and with evident advantage; endeavouring to judge with as much accuracy as I could, of the needful quantity, by observing the effects on the stomach.

*Putrid colluvies was frequently evacuated, during the whole course of the fever, by small doses of pulv. rhei, and its putrescency corrected by the exhibition of yeast.*

Elixir of vitriol was used during the whole progress of the fever; and paregoric elixir at night. Where the diarrhœa was excessive, it was restrained by starch injections with liquid laudanum: blisters were sometimes used, and sinapisms always.

The face, neck, hands and feet, were washed twice a day with tepid water, when the morbid heat on the skin was the greatest. Vinegar and water were sometimes used, and cold water, when the prejudices of patients would admit.

The bed and body linen was changed daily.

The rooms, in which the sick were confined, were kept constantly fumigated with the muriatic acid, by pouring ol. vitriol. upon pulverised common salt, placed in a tea-faucer, near the bed-side of the patient.

About 16 years since, a similar fever prevailed here. It was then undoubtedly introduced from abroad. About 20 persons had it at that time; evidently catching it one from another. Of this number 5 died.

The mode of treatment then pursued, if I am rightly informed, was a very stimulant one: viz. to give bark and wine, as much as could be got down the patient. At least this was the method pursued, with those who died. One of these patients took 14 gallons of Madeira wine, during a sickness of six weeks.

Between 40 and 50 years ago, a similar fever was epidemic at two or three different periods. It was treated by a physician who followed Dr. Huxham's plan. He gave, after evacuations, neut. julep. (*Quere?*) and Huxham's tinct. cort. com. and bitter tea. This physician was always timid in the use of stimulants, and was very successful.

These are the only periods, since the settlement of the town, that an epidemic typhus has been prevalent. The situation of the town is remarkably healthy as it respects fevers.

The scarlatina anginosa has been epidemic twice within 15 years; during which period, only two persons have died with the disease. And these were, I believe, almost the only two, who ever took bark and wine during the first six days of the fever. These were evidently injured by it. The fever attending the scarlet eruption and sore throat, is commonly thought to be typhoid; but still I must think cinchona not indicated in its first stages, at least in this climate.

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I wish to embrace this opportunity to add my testimony in favour of a particular formula of medicine, first recommended by Dr. Griffiths, and afterwards, in an improved form, by Mr. Jenkinson. See London Medical Journal, Vol. VIII. page 43. The following is my manner of making those pills, viz.

R. Pulv. gum. myrrh.	ʒij.
Sal. Mart.	ʒj.
Sal. tart.	ʒfs.
Gum. camphor.	gr. xvi.

Rub the camphor in a little spirit of wine in a mortar, then add the other ingredients, and make a mass for pills : to be divided into 36, and immediately rolled in flour or pulv. glycyrrhiz. Rather more than usual care is requisite to form the mass.

The dose is three or four pills three times in a day. This medicine I have found extremely useful, not only in pulmonary complaints, where tonics were proper ; but, in various instances of dyspepsia, hysteria, hypochondriasis, and other complaints where a light tonic appeared to be indicated. I think it a valuable improvement in pharmacy ; and as such wish to recommend it, to my medical brethren.

I am, Sir, with much respect,

Your most obedient Servant.

ELISHA NORTH.

DR. JOHN REDMAN COXE.

*Case of the Division of the axillary Artery, healing without being secured by Ligature.—Also an Account of a Case of Death, from a Wound of the Trachea and Oesophagus. By DR. JOHN VANCLEVE.*

PRINCETON, Feb. 7th, 1807.

SIR,

**I**F you think the following cases sufficiently important to merit a place in your Medical Museum, you are at liberty to insert them

Your Sincere Friend,

JOHN VANCLEVE.

DR. JOHN REDMAN COXE.

JUNE 21st, 1806, David Waters, aged seven years, while ascending a flight of steps, with a common glass bottle in his hand, fell upon the bottle and broke it; a long spicula of which pierced the armpit, and divided the axillary artery. The discharge of blood was so excessive, that upon the arrival of Dr. — (who was first called,) the boy was almost lifeless. There was no pulsation in the left, (viz. the wounded) arm; in the right it was perceptible, but very faint; the hemorrhage had now ceased. After the child revived, the Doctor removed the compresses, which had been applied by the family, and no blood following, a good opportunity offered for a minute examination. The external wound was situated transversely, about the middle of the hollow of the arm-pit, in length near an inch and a half. Internally it was found to extend upwards and inwards, in such a manner, that the glass passed near the upper rib. Although this examination was made with the greatest care, the Doctor was not able to discover the divided ends of the artery; he thought of dilating the wound, but was deterred from it, by the previous great loss of blood and the present extreme weakness of the child; he therefore concluded to dress the wound superficially, and wait until the return of the hemorrhage, before he would attempt any thing further.

22d. At one o'clock in the morning, in consequence of an effort to vomit the hemorrhage returned, when Doctor — and myself were sent for. We found the boy asleep and the hemorrhage stopped. The pulsation in the right arm was weak, in other respects it was natural; the wounded arm was cold and without any pulsation. As there was now no discharge of blood, we thought it prudent to postpone any examination of the wound, until we should have the advantage of day light.

The same day at 10 o'clock, we found the boy awake and cheerful; the arm still cold and without any pulsation, sensation, or the power of voluntary motion. We removed the dressings but no blood followed. After examining the wound as accurately as possible, we concluded to apply common superficial dressings in the usual way, with the addition of a compress, placed in such a manner, as to make a gentle pressure on the extremities of the divided artery.

As it was highly improbable that an artery of so great a size, and divided in such a manner, would heal without being secured by ligature, we determined to dilate the wound, and if possible to take it up; but deemed it imprudent to attempt the operation, while there was no discharge of blood; for the following reasons.

1st. If the division of the artery should prove to be so deep, or in other respects so unfavourably situated, as to render it impossible to secure it; the child might bleed to death under the operation. Should this unhappy event take place, we might be upbraided with having accelerated the death of the child; whereas, had the operation been performed during the hemorrhage, our characters would not be exposed to such disagreeable imputations. This unfortunate termination of the case, would be the more likely to occur, as we were intirely deprived of the command of the blood. The contiguity of the wound to the body, rendered the application of the tourniquet impracticable; neither could we compress the subclavian artery, on account of the position in which the child held his shoulder.

2d. No injury could result to the child by postponing the operation as long as no hemorrhage took place.

3d. As the limb was cold, and having in it neither pulsation, sensation, nor the power of voluntary motion, there was great



probability of mortification taking place, in which event, amputation at the shoulder joint would perhaps be necessary; which would afford a good opportunity, for making every necessary examination.

These reasons we judged sufficiently important to justify us in our determination, not to dilate the wound, until the return of the hemorrhage, or, until mortification should take place. Having dressed the wound in the manner above mentioned, and prescribed a low diet with occasionally gentle laxatives, to prevent too great an accumulation of blood, and having strongly inculcated the necessity of remaining absolutely quiet, and avoiding all bodily exertion, we left him.

23d. Continues in the same situation.

24th. A slight degree of sensation and warmth had returned to the arm and hand; in other respects the same.

25th. Removed the dressings; a copious discharge of matter rather thin and pale, but otherwise purulent, had taken place, I thought I could perceive a slight pulsation at the wrist.

27th. Continues the same, except the pulsation at the wrist, which was not evident.

July 2d. Still the same.

3d. To-day we were sent for in great haste, the hemorrhage having returned with violence. Before our arrival the discharge had ceased, and did not return, although we took off the dressings and moved the arm with freedom, hoping that the bleeding would commence, that we might have a plausible excuse for dilating the wound.

5th. A considerable discharge again took place.

After this, the wound gradually healed and was completely cicatrized in about four weeks. Ever since, the child has been perfectly well, except the arm having recovered only a slight degree of sensibility and warmth, and is still totally deprived of the power of voluntary motion. I have frequently examined the arm, but could not certainly discover any pulsation in it.

Our injunctions respecting rest and diet, were but imperfectly complied with. He was indulged with food, both in too large quantities, and of too nourishing a nature. He was also permitted to make too great bodily exertions.

1st. Had our directions been strictly adhered to; had the pulse been kept below par, by bleeding or otherwise; would the discharges on the 12th and 14th days, have taken place?

2d. Were our reasons sufficiently important for not dilating the wound? And had the wound been dilated, was it not probable that an insuperable difficulty might have occurred, in securing the artery?

3d. Does not the event of this case in some degree prove, that there is not that great danger to be apprehended, from the division of arteries of considerable magnitude, that is generally supposed; provided the patient be kept quiet and sufficiently reduced?



*Case of Death from a Wound of the Trachea and Oesophagus, with an hot Iron.*

April 18th, 1806. William Johnson being intoxicated, insulted a blacksmith, while engaged at his forge in making nails, The blacksmith made a thrust at him with his nail rod, and wounded him in the trachea and oesophagus. The perforating extremity of the rod had been recently heated, and the smith had just begun to draw a nail when the wound was inflicted.

The perforation was made in the anterior part of the neck, directly above the sternum, the hot iron being left in the wound; the adjacent parts were so much destroyed, that a small finger could be readily passed into the wound. The patient immediately lost the power of speech, on account of his breath passing through the opening in the wind-pipe. When the hand or any compress was applied to the opening, he could speak without any difficulty, but the air escaping into the cellular membrane, caused a great emphysematous swelling, which extended over the whole neck, face and a great part of the breast. His face was tumefied to such a degree as almost to blind him. When he attempted to drink, the liquid passed into the wind-pipe; as was manifest from the strangulation which was produced by it. As this was a case of an extraordinary nature, inasmuch as a part of the substance of the trachea and œsophagus was destroyed by the hot iron, no small difficulty occurred in the treatment of it. It appeared impracticable to dress it according to the directions generally given for wounds of those parts, on account of the destruction of so much of their substance. The wound in the œsophagus, I concluded rendered the case mortal, not so much from its extent, as from its nature. However I drew the sides of the external wound together by means of adhesive plaster; and applied so much pressure as would keep the external integuments in contact with the trachea to prevent the escape of air. The man being strong and plethoric, I took from him twenty ounces of blood. About 12 o'clock the following night, in attempting to swallow some liquid, he strangled and expired immediately.

Had the injury extended only to the trachea, might it have been looked upon as a curable wound? and if so, what should have been its treatment?

*A Case of Strangulated Femoral Hernia where the Operation succeeded, after the Obstruction had continued ten Days. By*

JOHN HAHN, M. D.

IN the evening of the third of October 1806, I was requested to see Michael Young, adult, who had laboured 24 hours under a strangulated hernia. On examination I found an irregular oblong tumor, situated on the anterior part of the thigh, directly over Poupart's ligament, of the size of a large orange, very tense, somewhat painful to the touch, attended with pain about the umbilicus; frequent vomiting, and some fever. The taxis and some of the collateral remedies, had been found ineffectual by the physician first called. A further trial of reduction was attempted, assisted by injections of tobacco fumes, warm baths, &c. To be brief, every method of reduction proved ineffectual. Symptoms becoming daily more aggravated, it was thought advisable to operate without further delay, on the fifth day after the strangulation; but by the persuasion, and the dreadful description given of the operation, by some of the patient's friends, he would not consent. After being made sensible that nothing further than the operation could be done in his case, we left him with directions to send, as soon as he could reconcile himself to have it performed.

To my great astonishment he suffered all the dreadful consequences of a strangulation for ten days. Symptoms had now arrived at the highest pitch; the pain excruciating, abdomen tympanitic, almost a continual hiccough, vomiting of excrementitious matter so frequent and offensive, that it required strong nerves to be near him.

In this state and late period of the disease, little hopes could be entertained of the success of the operation; but as death was inevitable, nothing worse could result.

As this case is submitted to print, not with an idea of giving any improvement in the mode of operating, I will content myself with mentioning the state of the parts, as they appeared in the different steps of the operation. After dividing the skin and fascia lata, the sac was found in a state of inflammation, rapidly approaching to gangrene, with firm adhesions to the surrounding parts. After a perplexing dissection, the ligament, with the sac protruding from under it with a narrow neck, were brought perfectly into view.

To remove the stricture in the tendon was the next object : Mr. Bell's method of dividing some of the external fibres of the ligament was attempted, without any benefit resulting from it, the tendon remaining as tense as before : I introduced with some difficulty my little finger, taking up the fold of the tendon, and bringing it forward, divided it on the tip of my finger ; this immediately admitted me to pass my finger freely between the sac and tendon. The last mentioned step of the operation, I could not forbear describing, as it is one which has caused different opinions ; but after several times experiencing the happy effects of the above method of dividing the tendon ; I flatter myself it may be recommended as one perfectly safe, and answering every end to the satisfaction of the operator.

After an easy passage for the finger under the tendon had been obtained, the contents of the sac remained irreducible. The stricture in the sac and whole extent of protruded peritonæum were now divided ; which, not to be extravagant in my expression, was three times its natural thickness ; the reduction was easily accomplished. From the description given of the external appearances of the sac, some idea may be formed as to the state of its contents ; they consisted of about an inch and half, or two inches of small intestine, resembling a piece

of scarlet cloth, and a portion of omentum equal to the size of an egg, bearing a far greater resemblance to a fragment of placenta, than natural epiploon; both adhering to the sac, by the intervention of coagulum; this being removed, the parts were reduced.

From a difficulty of bringing the lips of the wound over the sac, a few stitches were applied, and the edges partially drawn together; a few strips of sticking plaster, and pledgets of lint dipped in olive oil were applied. The day after the operation, the parts were fomented, and continued until there was a plentiful suppuration in the wound. In six hours after the operation, the favourable termination of this case commenced with a plentiful discharge from the bowels; these continued regular, no pain, fever, nor any dangerous, or disagreeable symptom occurred except the hiccough, which continued troublesome some days after the operation. By the above simple treatment, this patient was restored to a state of perfect health in seven weeks.

By the success attending this case, I do not wish to inculcate a propriety of putting off the operation in every case, to this late period; but offer it as an encouragement when we are unfortunately prevented from operating at the proper time, by the fears of the patient, or the inconsiderate persuasions of his or her friends.

*A Case of Chronic Rheumatism, cured by the Stimulating Impulse of a few Rounds at the Art of Boxing. Communicated by JOSEPH KLAPP, M. D. of Philadelphia.*

EVERY law which attentive observation may reveal, relative to so mysterious a subject as that of the animal economy, must in some measure promote the improvement of the medical art, though the imperfect state of our professional knowledge will for a while compel us to view the discovery in the limited light of a mere fact, which at present is aloof to every explanation.

When we contemplate the animal machine, endowed with that deep-rooted, and exceedingly abstruse power denominated sympathy, we cannot avoid being sensible of the superior wisdom of its great author, and at the same time feel conscious of the incompetency of our feeble talents, to develop many of his works.

I know of no one attribute of animal life, which is so completely hidden from the sight of investigation, as the consent between different and even remote parts of the organized body. In making this acknowledgement, however, I wish not to be understood to discard (as some among us affect to do) this very useful performer of many offices in the living system, as a banished tool for speculation; because I am really of the opinion that an acquaintance with it, has guided to much successful practice. The experiment of Dr. Hartley, in which the operation of nux vomica on the stomach, was intirely counteracted by severely whipping the dog to which the poison was given, at once disclosed the power of the sympathetic attachment between the skin, and the digestive organ. The much admired author of the *Zoonomia*, has recorded an account of a com-

bat between two dysenteric patients in the same ward of the Edinburgh infirmary, who horse-whipped each other for a long while, and the Doctor says they were much better after it.

The case which occurred under my notice, is an instance of the utility of what is commonly called a fisticuff drubbing, in the cure of chronic rheumatism; after many other remedies had been unavailingly used.

N. G. aged about 17 years, was under my care for a violent rheumatic complaint; the pains during two or three months, alternately affected almost every joint and limb of his body. The ordinary evacuating remedies were first used to reduce the inflammatory condition of the system. After the complaint had degenerated into the chronic state, blisters and external irritating applications were used, without being attended with the desired effect. Exercise in walking, and riding on horseback, together with electricity were found equally inefficacious. Being apprehensive, that I could not readily relieve him, the more violent remedies were omitted, and he was left to the charge of his mother, with the direction to take two tea-spoonfuls of the vol. tinct. guaiac. three times a day. In about two weeks after my regular attendance was discontinued, I called in, to inquire after my patient, when to my surprise I found him in the perfect use of all his limbs, and apparently recovering his health very fast. I immediately congratulated myself on the good effects of my last prescription, but upon questioning, I found that he had not used more than two or three doses of the guaiacum. His mother informed me, that Neal had met with rather an unusual cure of his rheumatism, and that was a severe battle which he had fought a few days since, with a chap that had greatly insulted him. Neal confirmed this statement of his



mother, by remarking that the affray had been of more service to him, than all the medicine which I had given him.

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*Observations on the Medical Treatment of the Croup.* By DR.  
THOMAS W. SMITH.

*Loudoun County, Vir. February 15, 1807.*

DEAR SIR,

I BEG leave to transmit to your perusal, the medical treatment of 18 cases of croup, which have come under my notice since the 1st of April 1805, which if you deem worthy a place in your Museum, are at your disposal. The cases were all noted as they occurred, and consequently their authenticity may be relied on.

While under the auspices of Dr. Baldwin of Winchester, numerous cases of this most formidable disease, occurred to his practice, and without the loss, as well as I can recollect, of a solitary patient. He omitted venesection entirely.

On my commencement of practice in this county, I adopted his practice with the same happy effect, until I was\* called in consultation, on a case of the same kind, where venesection was used and the patient recovered. I (then, with hopes of a more speedy recovery, as also to appease the anxiety of the distressed parents, who generally thought, as it was customary, nothing else could save life,) made free to perform the operation myself, and from that time a journal was correctly kept; from which the following is extracted without alteration.

\* I believe unfortunately for six of my patients.

	Age.		Venesection.	Recovered.	Died.
	Yrs.	Mos.			
M. Taylor's child	2	2	not bled.	recovered.	
T. Vilett's do.	1	6	b bled once		died.
E. Furr's do.	2	0	do. three times		died.
Mrs. Gore's do.	1	9	not bled	recovered.	
S. Holliday's do.	2	5	do. do.	do.	
E. Newlin's do.	2	5½	bled 4 times		died.
J. Gibson's Senior do.	2	0	not bled	recovered.	
J. Baldwin's do.	2	10	bled twice		died.
J. Waters's do.	3	0	not bled	recovered.	
R. Martin's do.	2	0	bled twice profusely		died.
do. do.	0	6	not bled	recovered.	
J. McFerfon's do.	7	0	bled once	do.	
R. Fletcher's do.	1	3	not bled	do.	
H. Leath's do.	0	4½	do. do.	do.	
J. Palmer's do.	1	3	do. do.	do.	
J. McDonald's do.	0	5	do. do.	do.	
J. Carr's do.	1	5	bled 3iii	do.	
J. Tutt's do.	0	5	not bled	do.	
Total 18 cases.			7 bled 11 not bled	13	5

These, Sir, are the few cases which I am able to furnish you at present, and that they were all cases of croup, maturely confirmed, need not be controverted; it being a disease whose characteristic symptoms will readily distinguish it from every other species of cynanche. Little however, did I think, when I noted them, they would ever become the subjects of your perusal. But observing the general success of physicians from the common treatment thereof, by venesection; and being firmly convinced of the propriety of omitting it, my anxiety has increased with my success, until I am at length urged to expose them in the uncouth manner in which they are presented.

In the treatment of the croup, my practice has differed very little from that recommended in the Medical Repository, and is as follows. When the symptoms are alarming, (as was truly the case in the above,) I gave to patients from 1 to 5 years old,

calomel ppt. grs. v. ad vii every two hours ; a strong decoction of rad. senek. serp. as much as was necessary to excite vomiting, and this as often as they were much oppressed with phlegm. Small quantities of the same, were repeated every few minutes during the intervals, to preserve a constant stimulus in the fauces. A blister was applied to the external fauces and afterwards dressed with mercurial ointment ; which latter also was rubbed freely under the arms, and sometimes calomel under the tongue, and on the gums. The warm bath two or three times a-day ; spt. nit. and ant tart. when the fever was high ; vegetable cathartics and anodynes occasionally. I have sometimes found a warm emollient poultice to the throat of service. I have exhibited the indigo solution, with very little or no effect. To a child under one year old, I seldom gave less than grs. iii, merc. mit. mur. or a larger dose than six or seven grs. to any age, I have not found to answer so well ; although several of the above patients, took from 60 to 80 grs. of mercury in twenty-four hours, no disagreeable effect was produced ; the salivary glands in none of them being highly excited. I have known three apparently dangerous cases cured by onion juice and honey, and a decoction of nettle roots.

The cure of this disease, no doubt consists in effecting a solution or separation of the membrane which is always formed in the trachea, by procuring a temporary relief from the symptoms by the decoction, while we endeavour, if possible to affect the salivary glands, and those of the trachea.

Believing the fever attendant on this disease, to be merely sympathetic, and to proceed more from dyspnoea and obstruction of blood in the pulmonary vessels, than from local inflammation, I have paid very little attention thereto ; always finding it to yield with the local or primary affection, unless a part

of the matter, forming the artificial membrane, was absorbed and proved an irritating cause, which was then thrown out on the skin in form of eruption, and mitigated the fever.

If then we reject venæsection as a nuisance in the treatment of this disease, which will appear by the preceding statement; in what manner shall we account for its ill effects. Adhering strictly to the principles embraced in my *Theory of Fevers\**, respecting the phenomena of sweat and vesication, I am induced to believe that all exudations succeeding violent excitement, either general or local, is the consequence of indirect debility, in the vessels of the parts from whence this effusion proceeds. That this is the case in abscesses and many kinds of hæmorrhages, no person will pretend to deny: hence the discharge may be called passive. If therefore blood-letting is admissible at all, it must be in the commencement; or in what I call its inflammatory stage, before respiration becomes difficult. After the membrane is formed, which in my opinion, seldom acquires aid from spasm to gain the conquest; it acts as a mortified to a sound part, or as an extraneous body, and excites the efforts of nature to relieve herself, of the offensive substance. Then how shall we assist her? must we, by venæsection, exhaust in a great measure that pabulum of vital energy, with which despondent nature is blessed; and thereby diminish those vital powers, which are so indispensably necessary to overcome the disease? Or shall we by stimulants, particularly such as maintain a specific action on the glands and exhalants of the parts, increase the excitement therein, (even to a slight degree of inflammation,) in order to throw off the offending substance; as in the cure of gangrene, &c.?

What indications led to that practice, which exhausts the excitement with one hand, and endeavours to restore it with

\* Medical Museum, Vol. II.

the other, I am at a loss to determine. The excitement I believe is seldom too great in infancy, to favour a mercurial action: there is no absorption required; no danger of death from fever. Neither is there any of gangrene.

The principal inducement to the practice of venæsection, perhaps, is the speedy relief, which is generally obtained by it; but let it be recollected, that this relief is always treacherous, as it is only a palliation of one or two symptoms, while the cause remains yet untouched, to combat a system now greatly debilitated by the operation, and consequently far less able to subvert its enemy.

I have never seen a case of cynanche spasmodica, wherein to make trial of the lancet, but should there be such a species of croup, perhaps venæsection might there prove serviceable.

I am, Sir,

Respectfully Yours, &c.

THOMAS W. SMITH.

DR. JOHN REDMAN COXE.

*Case of successful Treatment of Yellow Water.* By DR. JOHN STEVENSON.

NEWTOWN, Worcester County, Md. July 30th, 1806.

DEAR SIR,

**A**LTHOUGH I am not a subscriber, I am a constant reader of your Medical Museum; I wish it success, and am glad of any opportunity I may have, of contributing thereto.\*

\* Success in a work of this nature, requires the joint operation of subscription and communication.

I send you for insertion, the history of a case of YELLOW WATER, and its successful treatment. Many of our deductions and indications of cure, are drawn from analogous reasoning. This disease of the horse, will show its analogy to our autumnal remittents or yellow fever. The tracing the diseases of our own species into that of the brute, opens a field to our view perhaps too little cultivated: if therefore, you deem it not derogatory to your plan; by inserting it, you will probably awaken the pen of some one more able to cultivate this soil, and make it bring forth, thirty, sixty, or an hundred fold.

My riding mare, six years old, (very sprightly and active,) shewed symptoms of indisposition some days, after being rode hard in a cold N. E. rain about nine miles, and standing out of doors all the succeeding night of the 25th of September 1805; which I believe acted as a predisposing, if not an exciting cause. I will briefly describe the symptoms I discovered, and in the same order.

1st. A dull, heavy, sleepy look; inclining to hold the head lower than usual.

2d. Her gait corresponding with her looks; moving with apparent reluctance.

3d. A strong pulsation in the arteries of the neck; visible good part of their length.

4th. A hurried respiration, very apparent; especially after the least exercise.

5th. Frequent micturition; urine not perceptibly altered from its natural state.

6th. A great thirst: the tongue white, and mouth hot.

7th. A waiting of the flesh ; while the appetite remains unimpaired.

This symptom though last in its appearance, is the first that is generally noticed by our country people and farriers.

This being the season at which the yellow-water prevails, I immediately mistrusted it ; but was hitherto unacquainted with its characteristic symptoms ; and hearing of no well attested case, that was successfully treated, I felt a great diffidence in prescribing. I had had an opinion of my own, respecting its nature and treatment, viz. that bleeding and purging were its principal remedies ; but was deterred from putting it in practice, from hearing it so universally spoken against, by all farriers I consulted ; which by the way, were not a few. They all have a distinct mode of practice ; and an innumerable quantity of *different, secret*, and in their own words, "*infallible remedies*," which they recommended me to make trial of. No two of them agreed in but one thing, in which they all agreed : viz. *that bleeding and death were synonymous terms*. Under such opinions and such advice, I was tossed upon an ocean of uncertainty for several days, before any mode of treatment was adopted.

Give me leave to make a small digression here ; and acknowledge in a most public and solemn manner, that I adopted my opinion from a lecture I heard Professor Barton deliver, in the University of Pennsylvania, at the close of his course in 1797. I confess, I thought at the time, the accounts he stated of the immense quantity of blood that was taken in some cases, were considerably exaggerated ; as I think he intimated at the time.

I now began to conclude, that the want of success, or rather the mischievous effects of the fleam, were owing to its not being carried to an extent sufficiently great ; I at length came to a resolution to take the treatment on myself alone, and

purfue it on my own plan. Accordingly, I took away about an half gallon of blood, which my ignorance or timidity at the time, caufed me to think a pretty large bleeding. This was about the firft of October. She appeared to travel more lively afterwards; though the pulfations in the neck continued. She grew worfe in two or three days, the operation was repeated to about the fame quantity, but without any apparent advantage. I now quit ufing her, as I thought exercife might be injurious; ſhe was moreover fo dull by this time, I could ſcarce get her out of a walk. I continued bleeding her about twice or three times a week, to about a gallon each time, until the latter part of October. I was alfo anxious to purge her. I got a farrier in the neighbourhood to drench her; as I felt a confiderable degree of awkwardnefs; he gave lintfeed oil and molaffes a pint each, as one of the “infallibles,” it had no effect but to naufeate. I then formed an ounce of aloes into a mafs with foap, and gave it in two balls, night and morning, without effect. I then formed another mafs of one ounce of aloes, two drachms of calomel, and as much gamboge; divided it in three balls and gave one every 12 hours, after feeding the preceding day on bran; it barely loofened her a little, about 36 hours from the time the laſt was given.

As my aim is “pro bono publico,” it may not be amiſs to inform the young farrier how medicines of this kind may be eaſily given. I learned it from Taplin’s ſyſtem of farriery, the only one I ever have peruſed. Seize the tongue and draw it out of the mouth, ſideways, with your left hand; having your fingers of the right, gathered up in a conical form, with the ball between them, and keeping the head well elevated with the left; the ball is to be put in the back part of the mouth, and propelled a little on letting it looſe; inſtantly withdrawing the right hand, and letting go the tongue at the ſame time with the left, and clapping it under the chin to keep the head up; the ball is ſeen to paſs immediately.—But to return.



Her appetite now begins to fail, for the first time. I impute it to the last medicine; she has been wasting in flesh some days. The pulsation and all other symptoms continue. From the failure of her appetite and horrid gaunt looks, I conjectured she could not last long; she however would eat some. I gave her what she would eat best, which at this time was Indian peas; she appeared to have a difficulty in chewing grain of any kind; gathering it in her mouth and putting it out again; green fodder, Indian meal, and scalded oats and bran were alternately used. Thus I went on, until about the first of November, all my acquaintances and (what added considerably to my mortification,) the whole tribe of *horse-doctors*, began to laugh at me, and say "you have bled her to death." I was resolved however, to make one more effort. I again opened a vein, with a determination to keep the blood flowing until she fell, or began to stagger and reel about; I took two pots\* full, and then suffered it to flow on the ground some time, and no sign of syncope coming on, I began to think I should "bleed her to death" in good earnest. I removed the ligature, and applied it again the same day, and took nearly another pot full; I was governed by the visible pulsation entirely; I could sometimes feel the pulse in the fore knee-joint, but it was generally obscure. This last bleeding, had more effect on the pulsations and respiration, than any former two. The next day I took one pot full, and drenched her with a quart of a saturated solution of sea salt, night and morning, until

\* I always used a large chamber-pot that held one gallon, to receive the blood, in order to ascertain its quantity. The blood in every instance, except one, was taken from the jugular vein; and always had an inflammatory, buffy coat, from one to two inches thick, according to the size of the stream. I sometimes caught a portion in an eight ounce wide mouth phial, which generally shewed as much gluten, as crassamentum; or as much yellow as red blood: the line of distinction, always very plainly marked between them. This glutinous coat was so firm, that by thrusting a stick through it, I could frequently raise the whole mass by it. The size of the orifice had a remarkable effect, in shewing this buffy coat.

she had taken a gallon; it operated as a gentle laxative only. In exhibiting this drench, I discovered an offensive smell in her breath; which somewhat revived my spirits, as I concluded it was the effect of the mercury; and the difficulty or rather hesitation shewn in eating, served to confirm it; it did not however, any way resemble the smell produced by mercury on the human subject. There was no discharge of saliva. I now gave over exhibiting any more purging medicines; I will briefly observe, before I quit the subject, that a pint of new milk and another of molasses, was prescribed and exhibited for that purpose, by one of the "*infallibles*" without effect. I continued the bleedings, one gallon at a time, till the middle or latter part of the month; sometimes every day; sometimes once a week, according to the visible pulsations; which appeared to undergo some exacerbations without any evident cause. She was kept either in the stable or pound the whole time, with as much water from the well, as she would drink. I ought to have observed before, that I gave nitre in her food and drink, in the fore part of her sickness, in doses of about an ounce; and twice viz. night and morning 20 grs. of emetic tartar, without any perceptible effect. I much regret, I had not taken notes of all the particulars respecting the case. I well remember the doses of the medicines and the total number of bleedings, which were ten, to the latter part of November. I then had to leave home about a week or longer; she appeared better every way, except her gaunt looks and bad appetite, which had not yet begun to mend. I took about an half gallon of blood the day I left home, and she was bled by my directions, once during my absence; this was in the feet; the quantity not ascertained; making 12 times in all. I do not know that the last bleeding was necessary; but I thought it proper to err on the safe side. On my return, I found that the pulsations had ceased to be visible; her thirst had abated, her appetite had moderately increased, though still small: I gave two drachms of sulphate of iron, dissolved

in her drink or mixed in her food, twice a-day, about a week; her appetite increased under the use of it, she looks fiercer, her hair more lively, and she now began to wallow; which she had not done for two months. I was still not pleased with her eating. I gave her an half-pint of brandy, mixed with the same quantity of water; it had considerable effect on her appetite; I repeated it twice, which brought it nearly to its original standard. I began to use her moderately, early in January. On the 25th of this month, I was sent for in great haste, and rode her seven miles, and the largest half of the distance, in a full gallop. This produced a slight return of fever; one bleeding and a few days rest removed it. She has continued healthy, and has been in constant use ever since.

I have forgot to mention in its proper order, one circumstance, which, though not of much consequence, is still wanting, to render the narrative complete. Soon after, or about the time, her appetite began to mend, she discovered an inclination to eat dirt; to remedy this, I wet her food alternately in weak ley, soap-suds, and lime water; they did not answer my expectations; however, it gradually wore away.

These are all the facts and circumstances, respecting the case; as far as my memory serves me they are related with fidelity. I shall make but few observations on them.

Had I discovered the nature of the disease sooner, and adopted rigorous measures with it at first, and succeeded well in the use of cathartic medicines; I feel a degree of confidence bordering on certainty, that I might have effected a cure in half the time.

Whether it was owing to the great length of intestines, (which, in one horse I dissected measured 137 feet, without

separating them from the mesentery, more than what took place from stretching them on the ground,) or to their sensibility being absorbed by, and concentrated in, the blood-vessels, or whether the inefficacy of the purging medicines were owing to the feebleness of the doses; or whether their failure ought to be attributed to all these causes combined, I am not determined. Its analogy to the Yellow Fever, would lead to the second supposition.

The term *Yellow-water* is applied to this disease of horses with just as much propriety, as the term *Yellow Fever*, is to that, which at certain seasons, produces such havoc in most of our sea-port towns; and no more. I shall only remark respecting their affinity; that (as far as my information goes,) they both began, and have kept pace together in the United States of America.

The name of this disease, probably originated from the texture of the blood being broken down, from the long continuance of fever; which left to itself, or injudiciously or inertly treated, frequently terminates in effusions of lymph and serum under the integuments, and in some of the viscera. After death, and sometimes before, scarce any red blood is to be found in the heart or blood-vessels; nothing remains in them but a little gelatinous lymph or *Yellow-water*.

For my own part, I wish the diseases of horses were better investigated by those who are more adequate to the task. If any thing new should occur to me on the subject, I shall not fail to communicate it.

With sentiments of profound respect,

I am, Sir,

Your friend and humble servant,

JOHN STEVENSON.

DR. J. R. COXE.

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*Remarkable Fecundity of a Negro Woman.*

A negro woman in this neighbourhood has had seven children at three births; her first pregnancy was with twins; she had twins once since, with some single children between; her last was triplets, about three years ago. She is the mother of eleven children, and is about 38 years of age; was a twin child herself; all her twin children are alive and healthy.

Your's, &c.

J. S.

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*A Case of Dropsy cured. To which is added a very singular position of the Heart, with the Dissection; read before the MEDICAL LYCEUM of PHILADELPHIA February 11th, 1807. By JAMES SMITH, a junior Member.*

**J**ULY 19th, M. G. a black woman aged 31 years, was admitted into the Philadelphia Alms-House, with dropsy in the forms of anasarca and ascites. So great was the former, she was unable to flex her legs; abdomen very tumid, and fluctuation very evident; a little fever; pulse feeble, and she experienced great thirst. For the above disease she took one grain of squills, two of calomel, and fifteen of nitre, three times a-day. Her drink was wine and water: large rollers were applied to her legs and abdomen, and re-applied night and morning. This treatment, and above medicine, had the desired effect. It increased the quantity of urine very considerably, until a salivation took place. In a short time after, made her usual quantity of water. The swelling diminished day by day until all the symptoms disappeared. The ptyalism being kept up, by a

diminished quantity of the medicine, with the addition of half a grain of opium.

On the fifteenth of August, all medicines except wine were omitted, and on the 25th, she was discharged perfectly well.

It will be well to observe, that these symptoms took place in advanced pregnancy; but they were by no means relieved, on being delivered of a healthy child.

September 18th, she was again admitted a patient in the institution: (from her leaving it three weeks and three days having elapsed,) but with symptoms similar and more violent. Her respiration being very much impeded, which led to a conclusion of hydrothorax: without any hesitation, recourse was had to the medicines which before relieved her. The following being the prescribing day, of the much esteemed and indefatigable Dr. Church, on visiting the patient who was lying in a reclined situation, with the clothes thrown off her; he discovered at some distance a throbbing in the right side; could feel no pulsation of the heart in the left. At the wrists there was a thrilling of the arteries, similar to that of the side. The Doctor pronounced it an aneurismal tumour of the aorta, supposing death inevitably very soon would ensue. The medicines which before relieved her were omitted; at which time was prescribed, a mucilaginous mixture for a cough which accompanied the above affection; also wine one pint for three days.

On the 23d, it was pronounced an enlargement of the heart. She took pulv. digital. gr. one, three times a-day. The 25th the above medicine was omitted, in consequence of constipation and increased arterial action. Super tart. potassæ gr. v. jalap. gr. xv. were prescribed 3 times a-day. These being continued until the 30th, and having fulfilled their indication, were

omitted. At this time, she took tincture of digitalis, 20 drops three times a-day, mucilaginous mixture and wine.

On the evening of the fifth of October, she pronounced herself considerably better, and apparently was much relieved. But what more particularly excited attention, was a tremulous jarring pulsation, extending from the right side of the sternum laterally as low as the right mamma; and downwards as low as the eighth rib. On the morning of the sixth, she arose, dressed herself, and called for her breakfast. Having received it standing, some distance from her bed; in consequence of some agitation, she spilled a part of her coffee, at which she became very much irritated: and falling on her bed, died almost instantly without a struggle.

Examination being made by Mr. Stewart my colleague, in the presence of several gentlemen, the following appearances were very obvious. On making an incision through the cellular and adipose matter; the following appearance presented itself. The muscles having been entirely absorbed on the most projecting part of the abdomen. On continuing the incision through the peritoneum, a very considerable quantity of water was discharged. In all probability as much as ten or twelve quarts. The abdominal viscera presented an appearance tolerably natural, except the liver, which was much larger, and of a darker colour than usual.

On opening the thorax, the left lung presented itself, having an appearance nearly natural; possibly somewhat larger than is natural to the subject in a healthy state. The right lung, from the position of the heart, was perfectly secluded from view. The concave surface of the lung, which ordinarily embraces the right ventricle, now formed a complete bed for the heart: so that almost as much of the lung was situated on the



left, as on the right side of the heart. Cutting through the pericardium, a considerable quantity of water rushed out. This rendering it very uncertain, whether there was any effusion of water in the thorax or not. The auricles and ventricles, considered together, were larger than natural. The vena-cava ascendens and pulmonary artery, were considerably enlarged. The situation of the heart, with respect to the right side, was very similar to that, which in a natural situation it bears to the left.

How shall we attempt to account for the sudden death of this patient? Was it owing to the determination of blood to the head, by the stimulus of anger, and thereby producing apoplexy? Or can we suppose, by the same stimulus, from the distention of the heart it became disorganized? Because it was enlarged, we are not to suppose the valves were enlarged in proportion: But on the contrary, that they were incapacitated to prevent the flux and reflux of the blood: and from the distention it is probable a laceration might have taken place in the valves, and thereby produce a suffocated circulation. In this way I suppose she suffered so sudden a dissolution. Again, in what way shall we explain the thrilling of the artery at the wrist, to what Dr. Hunter calls a true aneurism, or dilatation of the artery? I cannot conceive this to produce that effect. How are we to explain this very singular and striking phenomenon, viz. the position of the heart, in occupying a relative situation in the right, to that of the left side of the thorax? To this I will offer a few rude conjectures. Is it reasonable to suppose any mechanical violence done to the body, could have changed its position, or could gravity, from its great collection of water in the pericardium, influence it to assume that position?

J. SMITH.



*An Account of the Morbid Effects of Lightning, upon the Human Body, successfully treated. By PHINEAS JENKS, M. D. of Newtown, Bucks County, Pennsylvania; communicated in a Letter to DR. RUSH.*

*February 25th, 1807.*

DEAR SIR,

I BEG leave to send you an account of the cure of a violent and dangerous disease, brought on by LIGHTNING, by means of FRICTION, the WARM BATH and BLOOD-LETTING. The remedies were as successful, as I believe them to be new, in this case. If you should think that it merits any attention, you are at liberty to insert it, in the Medical Museum: the Editor of that valuable work, having expressed a wish in one of his numbers, that physicians who may have an opportunity would record such cases.

I am, your sincere and

Grateful Friend,

PHINEAS JENKS.

BENJAMIN RUSH, M. D.

On the 20th of June 1806, we experienced the most severe thunder storm I ever recollect to have witnessed. While the storm was still raging, I was called upon to visit Mr. Y. about two miles from this village; who I was informed lay apparently lifeless, from a stroke of lightning. When I arrived, I found that his situation had been pretty accurately described to me; and from the length of time that had elapsed since the accident, I had but little hopes of a recovery. Upon examination, it appeared that the lightning had struck, first upon the crown of the head, and then passed down the side, breast, and arm; in some places destroying the skin entirely, in others producing extensive blisters. His body and extremities were

quite cold; he laid entirely insensible, with little or no pulse. Having never seen a case of this kind, nor reflected much upon it; while on my road there, I determined to make use of those remedies, recommended by authors; particularly cold water, so highly commended by the Royal Humane Society. But I no sooner saw the patient, than I was convinced, that this remedy was inadmissible in the present case.

I therefore had the patient immediately wrapped up in flannel, and frictions with ardent spirits, applied to the body, generally, with a view to increase the languid circulation, and produce if possible, a re-action of the system. But deriving little or no advantage from this remedy; the next that presented itself with any prospect of success, was the warm bath. From the general coldness and languor of the system, I did not hesitate to apply it. A bath was accordingly prepared of a moderate degree of heat, in which I had the patient placed, for the space of 20 minutes; during all this time the frictions were continued. He was then taken out, his skin wiped dry, and placed in bed. The good effects of the bath soon appeared evident, his body and extremities became warm, and a general perspiration ensued; his pulse became tense and somewhat full. From the tension in the pulse, I determined to bleed him: 12 oz. blood were accordingly drawn with considerable advantage; his recollection began now to return, and he had so far gained the power of articulation, as to make me understand that he felt a very severe pain in the breast, side, and one arm. This pain in a short time became so excruciating, as to draw from him the constant exclamations "I must die, I must die." The pulse rose considerably after the bleeding, and became more tense. From this circumstance, I was led to consider it a case of suffocated excitement; such as frequently occurs in pleurisy and some other diseases; and that by partially unlocking the system by bleeding, I had produced the pain, which was to

be relieved only by a repetition of the same remedy. I therefore, in the space of one hour drew 12 oz. of blood more, which relieved the pain considerably, and enabled him, for the first time since the accident, to swallow.

The symptoms appeared generally alleviated for the space of one hour, from the second bleeding; at which time they returned with as much violence as ever, and I was induced to take away 12 oz. more of blood, with the effect of relieving him a third time. After this last bleeding, his pulse became much slower and softer; although he still had some pain during the night and continued very restless. Thus in the space of three hours, I drew 36 oz. of blood from my patient, with the most happy effect.

June 21st. The pain in the breast, side and arm, has returned with some violence; the pulse is hard and tense, and the patient very costive. Directed a strong dose of jalap and calomel. I called again in the evening and found him much better. The physic had operated well, and the pulse was quite natural, although he had slept none during the day. I directed liq. laud. gtt. 25. and if it did not produce sleep in one hour, 15 gtt. more.

June 22d. At one o'clock in the morning I was sent for in haste, I found that he had slept none and was very restless; with high fever, and severe pain; (produced as I supposed by the premature use of the laudanum;) pulse hard and tense. I immediately drew 15 oz. of blood, which lessened the fever, and removed the pain; when he immediately fell asleep for the first time since the accident.

June 23d. The fever pretty high, with but little pain; directed the antimonial powders every two hours through the day.

June 24th. The fever much less, the powders continued.

June 25th. No fever, but great direct debility; directed wine, bark and nourishing diet, which I continued until he had recovered his strength. On the 28th he was so far recovered, as to walk over his farm, and give directions to his workmen; although he still complained of a numbness of one arm, which was speedily removed by the shower-bath. The burns were readily healed by means of the common remedies.

From the preceding case, the following conclusions may be made.

1st. Where electricity produces sudden death, it is by destroying the excitement of the system, by the excess of its stimulus; as sometimes takes place from a concentrated portion of miasmata. For all stimulants when applied in excess have this tendency, to a greater or less degree; hence their effect is an unit; and,

2dly. Where death is not immediately induced, there is so great a prostration of the system; that depleting remedies are dangerous, until gentle stimuli are given to produce a re-action; after which they may be employed with the happiest success. We have an example of this, sometimes, in violent cases of apoplexy, and in the case above recited.

3dly. In those violent and dangerous cases, there exists what you have happily called "suffocated excitement." To know this, is of immense consequence; inasmuch as the state of the pulse, without the knowledge of this circumstance, would deter us from the use of those remedies, which are alone calculated to cure.

*A singular Case in Midwifery. Communicated by LYMAN SPALDING, M. D. of Portsmouth, New-Hampshire.*

ON the 17th of February, 1805, I was called about 10 o'clock, P. M. to Miss Susan Stagpole, of this town: she had been restless and uneasy in the fore part of the evening; true labour pains had now come on. By examination I found it was a footling case, and that the waters had just broken. The labour pains increased in strength and frequency as usual; but the child did not advance so fast as I expected, from the length, frequency, and strength of those pains.

When the hips were expelled, I found the child was held by something in the womb; the pelvis was so very large that the child could be easily moved from side to side, not a little resembling the pendulum of a clock. The pains continuing strong, I brought down first the left, and then the right arm, but with some difficulty. The child presented with its face to the pubes, and occiput to the perinæum. At this time the child hung by the head and swang from side to side. I introduced my hand but could not positively discover the difficulty; and as the child was advancing rapidly by every pain, I thought best to wait a moment; at the next pain the shoulders were expelled. I again introduced my fingers to lay hold of the chin and bring that down; when to my great surprise, instead of a regular chin and mouth, I found something resembling the head of a child. I passed my fingers round, but before I discovered the true situation, another pain brought into the world, the heads of two children, locked together by their chins; with their faces resting on the fore part of each other's neck and breast.

The first, a footling case, with the face towards the mother's pubes; the second, a natural head presentation, with the face

towards the mother's perinæum. A slight pain or two expelled the second child.

The first, a male child, appeared to have been strangled in the passage, as the funis beat strongly when the breech was born. The second, a female, was alive, but appeared so feeble, that I did not think she would live an hour; she however gained strength, and is now a healthy child.

There was but one placenta, which was very large; the two cords entered at equal distances from each other, and from the edges of the placenta.

The children were as large as is usual for twins, neither were their heads of a diminutive size. The woman was 24 years old, had had one child before, and now had a good getting up.

The extraordinary size of the pelvis was astonishing; she had not more pain than is usual, and was delivered before 12 o'clock that night.

The head of the female, or second child, was expelled first from under the pubes; while the head of the first, or male child, rested in the perinæum, and was expelled at the next pain; so that in fact, the internal and external ossa, were stretched to nearly twice the bigness that would have been required for the expulsion of a single child; as in this case, both heads passed the pelvis at the same time.

So rapid was the descent of the children, after their heads had got into the pelvis, that even if I had suspected their entanglement, I doubt very much if I should have been able to disengage them.

*Observations on several Phenomena of Diseases, peculiar to Somerset, in Pennsylvania.* By DR. JAMESON.

GETTYSBURG, Feb. 13th, 1807.

DEAR SIR,

IF you think the following observations worth a place in your Museum, you will please insert them.

I resided at Somerset, Pennsylvania, from 1797 to 1799; during this time, I became acquainted with several phenomena, peculiar I think, to this part of Pennsylvania. I never saw a case of fever, except such as was strictly symptomatic. The salubrious air, is a specific against agues, remittent fevers, &c. I recollect two gentlemen who had been harassed so long, and severely, with agues at Fort Cumberland, as to render their very existence a burthen, to recover their health and strength very soon, after settling at Somerset. And yet what ought to seem strange, to the advocates of imported contagion, is, that small-pox exerts its baneful effects, on the inhabitants of this place, as in any other part of Pennsylvania; and well they know, that neither latitude, climate nor frost, can stay the operations of diseases, marked by the great Creator, with the awful character of contagious. I am of opinion, that no disease is contagious that affects the same subject more than once; if we were subject to repeated attacks, of small-pox and measles, like we are to yellow fever, &c.; soon would the world be depopulated, and the justly admired and beautiful works of man, dwindle to nothing, and leave creation as devoid of a creature to till the ground, as on the fifth day of creation!

Measles visits them also; the disease was not prevalent while I resided there; but, from the inhabitants, I learned that it



is the same disease, in its nature and force, that it is every where else.

The inhabitants are very subject to rheumatism, in all its various forms; and are also much afflicted with hypochondriac affections.

I am decidedly of opinion, that women in labour suffer much more, than in any other part I have practised in; distressing laborious labours are very common. What connection can there be, between rheumatism and these severe labours? I have always found a rigidity of the uterus, and other parts concerned in parturition, to be the obstacle to delivery. Since reading Dr. Dewees' remarks on blood-letting, I am of opinion, that bleeding would be a most invaluable remedy, for lessening the pain of those suffering women; I seriously recommend this to any gentleman, into whose hands this may fall, practising in that neighbourhood.

The complaint called yellow gum, I think, uncommonly prevalent in this place; I am of opinion from inflammation produced in the skin, in consequence of the long confinement in the passages: does this argue any thing in favour of the yellowness common in malignant fever, being the effect of heightened inflammation in the skin, and not arising from absorption of bile?

There is something singular in this climate; maize has not an opportunity of ripening once, perhaps in ten years; the farmers do not cultivate it, and yet, within a day's ride in all directions, it does very well. The principal produce of this place, is wheat, rye, potatoes, flax, and oats; which last, are much superior to any other oats I have seen. Fruit is very scarce, and never good. The apple trees, and oaks, in many places, are constantly covered with a verdant moss; which at a distance displays to the traveller, a fallacious appearance of spring,



whilst yet, dreary winter, paves the way with ice. The country is pretty hilly, but through it are many little waters, which glide through very extensive swamps, which, in my opinion, want nothing but a little more warmth, to render them prolific sources of pestilence. It is common to have frost, nine or ten months of the year; a gentleman of veracity told me, he had got ice in his meadow, in August; and I have seen Fahrenheit's thermometer at  $97\frac{1}{2}$  degrees, in the shade: one should suppose a climate so changeable must be unhealthy, but the fact is otherwise; for, although rheumatism, &c. prevail pretty much, yet they are comparatively a happy people, as they are exempt from the long train of diseases, originating from miasma.

I am, Dear Sir,

With esteem, your's, &c.

HORATIO GATES JAMESON.

DR. JOHN REDMAN COXE.

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*Case of Prolapsus of the Colon, terminating in Death. With the Appearances on Dissection; By DR. WILLIAM RUNKEL, Communicated to the EDITOR.*

I WAS called to see James Young, aged 23 years, a labourer, with a prolapsus of the colon. He had for some days laboured under a diarrhœa, accompanied with tenesmus, but still not so violent as to prevent him from attending to his work. A few hours before I saw him, the colon had come down to the length of nine or ten inches; and by the time I saw him, it was in a high state of inflammation and swelling: there being no possibility of reducing it, I ordered emollient applications to the part, and a proper support, expecting by the next

day, to be enabled to reduce it; but to my astonishment, I found it much larger than the day before; measuring in length about 12 inches, and in circumference about ten inches. I still made an attempt to reduce it, but all to no purpose. At the request of a medical friend, whom I had called in, the emollients were continued, but all to no purpose. Hickup and vomiting by this time had taken place: he was put into the warm bath; astringents were then applied, and attempts made with very long pipes to give him an injection, but they failed owing to the stricture of the part; castor oil was then given in large and repeated doses, which at last had the desired effect: all this time, the patient complained of violent pain in the umbilical region; the hickup and vomiting continuing; the pulse from the beginning, small and irregular. On, or about the eighth day, some part of the colon became sphacelated, and on the tenth sloughed off, a small ragged part remaining. The part that had come away, measured 24 inches in length: his bowels were once in 24 hours moved by oil; on the 15th day small symptoms of tetanus appeared, and increased until the 17th, when he died.

On opening the body, we found a general adhesion of the omentum to the intestines; and in fact a general adhesion of all the parts, and in a very high state of inflammation: the arch of the colon very much distended, and also in a high state of inflammation; and an abscess in the coats of the rectum, and some sphacelation in the lower part of the colon.

*Germantown, March 9th, 1807.*

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# MEDICAL MUSEUM.

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VOL. IV.....No. II.

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*Reply to DR. PATTERSON of Londonderry, on the Subject of CHOREA SANCTI VITI. By JOHN REDMAN COXE, M. D.*

IN the 15th volume of the London Medical and Physical Journal, Dr. Patterson of Londonderry, has published an answer to a communication of mine in a preceding volume of that work, extracted from the 8th volume of the New-York Medical Repository; entitled "Observations on Chorea Sancti Viti, with a new Theory of the Disease;" in which the Doctor accuses me of having censured not only some of his reasoning, but also part of his practice "in a manner at once abrupt, rough and magisterial." Conscious of having never intended to give the slightest offence to Dr. P. in that essay, (of the truth of which I request every gentleman to convince himself by a candid perusal of the piece); I should not now have answered his illiberal and unjust aspersions, had he not called upon me "for refutation or concession."

It seems extraordinary, that such harsh epithets should be applied to me, because I ventured to differ in opinion from my learned opponent, by saying, "it appears to me, that Dr. Patterson has greatly erred, in supposing chorea the primary disease; of which each must judge for himself." I see nothing

“abrupt, rough or magisterial,” in thus allowing every one to form his own opinion of the subject in dispute, after comparing the arguments on both sides. Surely, I have the same right to my sentiments that the Doctor has to his : and he will find it difficult to persuade mankind that *his ipse dixit* is to carry conviction to the breast of every reader ; or, that an individual may not be allowed the privilege of dissenting from him, without being presumed to act from unworthy motives !

The Doctor supposes I have censured his practice, by saying, (in referring to the case he relates in his letters concerning the internal dropsy of the brain,) “the operation of the emetics probably hastened her dissolution, by determining too forcibly the circulation to the brain,” (an expression he seems to sneer at,) “and hence exciting an acute state of hydrocephalus.”

Whilst I still adhere to the same opinion, justice impels me to absolve myself from any idea of imputing to the medical attendant any blame, from the administration of the emetics. I had not read the Doctor’s remarks with so little attention, as not to have noticed that their exhibition was not his fault. The manner of expressing myself, might to a candid mind have confirmed this ; for (although Dr. P. appears to be unacquainted with that urbanity which should characterize medical men in their opposition to each other’s sentiments, yet, knowing how liable we all are to mistakes,) had the Doctor been a principal on that occasion, I should have been peculiarly careful of speaking in a manner, to wound, either his feelings or his reputation. I should not, so virulently as he has done, have exclaimed,

“Quot Themison ægros autumnno occiderit uno.”

Considered however in a practical point of view, I must oppose, as far as an individual can, the use of emetics in those complaints, especially when increased action of the vessels

exists; although the practice may be sanctioned by "eminent professional characters."

"Nullius jurare in verba magistri."

Even several of the gentlemen he has named, are guarded in their expressions, by the terms, *gentle* and *mildest*, in recommending the use of emetics.\*

\* That the Doctor does not himself approve of the use of emetics, I think we may fairly deduce from his own words, although he has ventured in his answer to quote the authority of several medical men in their favour. "If Dr. Quin" (says Dr. P. p. 6. of his letters), "had felt even a faint conception of the present theory, would he in this case have prescribed, early in the disease," and if not early, why in its most advanced state? a remarkably strong vomit of ipecacuanha and antimonial wine?"—And in speaking of errhines (p. 52.) he says, that before using them, "precautionary considerations are very necessary; our subjects are tender, irritable, and affected with plethora of the head, the particular part on which these medicines operate, and to which they give such a degree of commotion, as would, *no doubt*, prove highly pernicious in the incipient stage of the disease. Nay I am not clear, that they are perfectly safe in the more advanced periods, when vascular repletion may be diminished, and serous effusion on the approach, or in any degree existing. For I apprehend, that the shock of sneezing, as well as the *agitation* of vomiting, if *by chance* eventually *harmless*, would not promote absorption in the advanced torpid state, and would be a vexatious experiment to the patient, to whom the smallest motion of the head is generally so great an annoyance."

In speaking of the administration of cathartics, he says, (p. 34.) "For a stimulating purgative must either be in such quantity, or of such quality, as would in many cases, especially where the stomach is so frequently disordered, prove actually emetic; an effect, which, you, (Dr. Quin), *properly observe*, would *hasten considerably the progress of the disease*, and I would add, an effect which must be highly pernicious at its commencement." He afterwards says, (p. 35.) "Touching vomits we may here remark, that several years ago, it was started as a question, whether they be, or be not, safe medicines in febrile disorders, *caput petentes*? Applying the question to hydrocephalus internus, which I reckon of a febrile nature, and most certainly *caput petens*; I would answer, that here emetics *are in my opinion injudicious remedies*, especially in strong doses at the beginning of the malady. And I ask, would you give them at this period, or indeed *at any other*, upon the principle of promoting absorption, by the general concussions arising from their operation?"

The Doctor thinks it behoved me, before pronouncing that he had greatly erred, &c. to refute the reasoning on which his conclusion is founded ; at which, however, he says, I have not made the smallest attempt, “ the performance of which is so necessary to vindicate my dogmatism.”—Whilst, however, a doubt remains on my mind, whether the Doctor comprehends his own reasoning, if such it can be termed ; I should recommend his looking at home for a plentiful crop of dogmas dispersed throughout all his writings. His “ innate nervous stimulus,” and “ principle of irritability,” which he so confidently tells us, “ is not confined to the fibre, or simple solid ; but is also possessed by the fluids of the body, particularly by the blood ;” I conceive to belong to this class ; unless indeed, the Doctor has better reasons to confirm them, than mere assertion. His assertion, that the heart “ *must be* the centre of the principle,” I regard as equally unqualified and devoid of proof. Of its “ influence on the contents of the cranium,” I desire no other proof than the effect of the emetics in the case alluded to.

The Dr. says, “ As in those ages, in which this constitution prevails, convulsions likewise prevail, the increased action which they produce, will augment the impetus of the circulation, especially in the vessels of the head ; which we see is actually the case, since a considerable determination to that part is observed in those disorders. This determination increases the action of the blood-vessels, and inflammation is the consequence. This consequence is forwarded by the plethora, particularly of the head, which exists in the early and advancing stages of life ; and the whole train of symptoms constituting *febricula hydrocephalica*, (The name which I appropriate to the disease,) is then brought into view.\*”

\* It may here be observed, that in the history which Dr. Patterson gives us of this disorder, (hydrocephalus) after enumerating the symptoms of the first and second stage, he ends those of the third stage, (p. 11.) by “ impaired deglutition, and convulsions, which form the catastrophe.” I certainly might with as much reason say, that these final convulsions of life, were the cause of the effusion of water, which dissection shews in the ventricles, as the Dr. has for asserting chorea,

I perfectly agree with him in the effect which is produced by the determination to the head, of inflammation, and of the whole train of symptoms, constituting febricula hydrocephalica, (as Dr. P. expresses it); but I view the convulsions, which he supposes the *cause* of the increased action, as the *effect* of the plethora, or effusion induced; for that hydrocephalus, induced by inflammation of the brain, should, by pressure on the nerves, (or their origin,) induce convulsions, (especially as it is in a part incapable of distention); is surely a more rational view of the subject, than that the convulsions should excite the determination to the brain, (which he only asks to be conceded as the *petitio principii*,) and the consequent febricula hydrocephalica. I would only ask, which is the most simple of these doctrines, or whether all the Doctor's reasoning on the subject amounts even to a presumptive proof of it?—The Doctor assigns not even a probable cause for the convulsions; but I have, I think, rationally proved the legitimacy of my deduction; and might add, that every case of injury to the brain, from blows, falls, and the like, tends to establish it; inasmuch, as in those instances where death takes place after convulsions; dissection has evinced the cause, to be matter formed, or water effused, or great engorgement of the vessels of the brain.

(which he afterwards admits to be merely a *species* of convulsions,) to be its origin. I think too, it may not be irrelative to state, that in the view of the different kinds of inflammation which the Doctor has taken to aid him in the investigation of the subject; he adds, p. 19 “No doubt there is a propriety, and even an advantage in distinguishing with accuracy, the organ immediately affected by inflammation; yet it is equally certain, that, in all such cases, the *great diversity* in the symptoms is *more imputable to a difference in the function of the part engaged*, than to any specific variation in the nature of the inflammation.” I think, although this is applied to the effect of inflammation, it is not asking too much of the Doctor, to impute the same diversity of symptoms, (that is, the various forms of convulsions,) to a difference in the function of the part engaged, by the greater or less pressure of a fluid, or turgid vessels in the brain, on one part more than on another, at different times. For we cannot but admit that pressure from the same source, will produce a different effect on one part of the brain, from what it does on another.



“But how are we to explain (fays the Doctor,) the occurrence of effufion, where the accumulation of the irritable principle is not fo vigorous, and yet the action of ftimuli is affiduous? In thefe cafes *I would conjecture*, that the nervous ftimulus, acting, *as I fuppose*, on a mafs of irritability lefs moveable than in convulfive habits, excites a paffion, which, if violent, fuddenly deftroys irritability, or the vital principle, and death enfues; or which, if moderate, engages only a part of the irritability, and the natural ftate of temper is regained for a time.” And he illuftrates this (reaoning!) with an account of the diffection of Mirabeau, who was found to have “un petit épanchement dans le cerveau;”—adding “does not this inftance fhew, that an irritable temper, almoft constantly exercifed, may be a caufe of effufions in different cavities, and of a watery effufion in the brain; which conditions, *in all probability*, were preceded by more or lefs inflammation in the diaphanous membranes of thofe parts? And muft we not admit an irregularity of temper in thofe perfons peculiarly fubject to hydrocephalus internus?”

I am forry to fay, I fee nothing like reaoning in the above conjectures and fuppoftions. What does the example of Mirabeau prove? What, if an irritable temper almoft constantly exercifed, is a caufe of effufion in different cavities; and even if preceded by inflammation in the diaphanous membranes of thofe parts? it does not prove convulfions, (unlefs convulfion and irritable temper are fynonymous,) to have excited it. Mirabeau was not fubject to thefe; although, it is probable, had he lived, the increafing effufion might have induced them. Neither can I fee the neceffity of admitting an irritability of temper, in thofe perfons peculiarly fubject to hydrocephalus internus, more than in any other fpecies of dropsy. I would, however recommend it to the Doctor, (if his theory is juft,)



to guard against that temperament so conspicuous in his writings, lest it should be followed by the same fatal effect as in Mirabeau.\*

I thought I had been sufficiently explicit in attempting to explain, how a small additional effusion, in a case of chronic hydrocephalus, might render it similar to a case of acute; for it is certain, that if the brain is capable of containing by slow increments, *tsi* of fluid, without a fatal issue; a small addition to this, suddenly made, though only to the amount of a tea-spoonful, might nevertheless induce all the symptoms of an acute attack, and even death itself; for the brain being incapable of distention, must have time afforded it to accommodate itself to the effect of pressure, by absorption of a part. There is in this nothing incomprehensible, nothing obscure; and I must still declare my firm belief, that the Doctor's patient fell a victim to an attack of acute hydrocephalus, induced by the operation of the emetics. At least, if a greater effusion of water did not take place suddenly; the dissection shews such an increased determination to the blood-vessels of the brain, as was fully adequate to the final catastrophe.

The Doctor proceeds to say, that "the influence of convulsive affections in causing accumulations of the fluids in the brain is not a new point in pathology; it has occupied the attention of physicians during some years past. A child was indisposed about 2 months with frequent head-ach, which was supposed to proceed from worms, but anthelmintic medicines afforded no relief, and he died in a convulsive fit. On opening the

\* It might not have been amiss for the Doctor to have added the small residue at the end of his *reasoning*, as it contains more sense than all the preceding. "To promote this end, namely, the extension of practical knowledge, an accumulation of facts is, undoubtedly the aptest means." If the Doctor could have viewed my cases, as a small addition to the facts elsewhere enumerated; he might have spared his ungenerous remarks.

head, the vessels of the brain were observed to be uncommonly turgid, and in the ventricles was found more than double the ordinary quantity of serum."—"In this case, (says the late Dr. Percival, of Manchester,) I apprehend the turgescence of the vessels was the *effect*, and not the *cause* of the convulsions; for the reflux of the blood from the head to the heart being obstructed during the fit, in which I believe the patient expired, the vascular distention must have been permanent. The redness and even the blackness of the face, which takes place in convulsions, affords sufficient proof of sanguineous accumulation;—"hence it appears, that the same speculative opinion, which Dr. Coxe styles a *new theory*, with respect to convulsive motions in hydrocephalus, was conceived at least 13 years before Dr. Coxe wrote, and had obtained so much credit as to induce the learned Dr. Percival to counteract its extension."

Although the attention of physicians during some years, has been occupied by reviewing "the influence of convulsive affections in causing accumulations of the fluids in the brain;" it by no means follows that they were correct in their conjectures. On the contrary, I think every fact tends to shew that, (as Dr. P. has done), they have mistaken the cause for the effect; a circumstance by no means uncommon.

The case of the child mentioned above, is clearly in point. The head-ach for two months might have led to a more rational deduction than that it proceeded from worms; and the uncommon turgescence of the vessels of the brain, after such a long continued head-ach, might have, I apprehend, led Dr. Percival to regard the convulsive fit of which he died, as the effect of such turgescence; and I doubt not, *to be* in no wise singular in this opinion, although it is opposed to the senti-

ments of that truly eminent Physician. I muſt ſay I think Dr. Patterfon ſtrives hard to give my opinions to another, when he aſſerts that Dr. P. had thought it neceſſary to counteract its extension at leaſt 13 years before I wrote; I ſee no author of that time, quoted as bringing forward the doctrine; nor any credit which it had then obtained, to induce Dr. P. to counteract its extension. Had the Doctor really conceived it 13 years before; or had it acquired any credit, the extension of which he might think it neceſſary to counteract; is it probable he would have ſaid ſo little upon it? And is it not ſtill leſs probable that Dr. Patterfon, in his expreſs treatiſe on the ſubject, and with this caſe before him, ſhould have paſſed it in ſilence, and not till now, when he thinks it will answer his purpoſe, detect a theory in Dr. Percival's writings, which any man of common candour will allow to have no pretenſions to it? If even I was to admit the fact as ſuch,—Dr. Percival is ſtating a caſe of convulſions, to which he had not approximated chorea, (the diſeaſe I have particularly treated of, as ſymptomatic of the affection of the brain;)—and, until Dr. Patterfon ſhews that Dr. P. intended his ideas to extend to chorea, *then* viewed as a primary diſeaſe, he muſt excuſe me for ſtill upholding my claim to the new theory, as he (contemptuouſly) ſays I ſtyle it.

“If, as Dr. Coxe ſuppoſes,” ſays Dr. Patterfon, “chorea be not an idiopathic diſeaſe, but ariſes *ſolely* as a ſymptom of the *chronic* hydrocephalus, the characteriſtic phenomena of the latter ſhould always precede, or accompany, the appearance of the former. Whereas, upon a careful examination, we ſhall find, that the ſymptoms of chorea exiſt without any token of hydrocephalus, as ſtrikingly exemplified in the caſe of my patient, already alluded to; in whom its eſſential features pre-

ailed, long before it wrought the change which terminated in the fatal effusion into the ventricles of the brain". The case above alluded to, examined attentively, will, I think most satisfactorily prove that hydrocephalus is a cause,—not an effect, of convulsions. The brain by long and slow effusion, (as was the case with his patient,) is enabled to accommodate itself to a pressure, which produced more rapidly, would induce death; witness the numerous cases related by authors. It is strange that Dr. P. should be so attached to the doctrine he holds, as to overlook the objections against it, and to bring forward cases strongly opposing it! I should be disposed to rest the merit of the question on this case, and doubt not that it will convey conviction to the minds of many, who perhaps at present, receive without reflection, the Doctor's assertions for proof.

"Had the cases of chorea, (says Dr. Patterson,) which came under the notice of Sydenham, been accompanied with hydrocephalic symptoms, so careful an observer certainly would have mentioned such striking occurrences, and would not have found as he did, those cases liable to periodical returns, which rarely, or never, happen in any species of hydrocephalus."

Had Sydenham been acquainted with the *acute* state of hydrocephalus, I think there is little doubt, that so sagacious an observer would have drawn an accurate contrast between it, and the chronic state; and might also, from the knowledge it would have impressed upon his mind, have been led to investigate more thoroughly, the sources of chorea, on which he has so slightly treated. The reason is evident why Dr. Sydenham did not detect the symptoms of hydrocephalus; because in the chronic state, many are wanting (owing to the effect produced by habit, as I have before attempted to explain,) which are almost essential to its acute attack. The wonder

is lefs, that Sydenham fhould have failed in demonstrating the caufe of a difeafe he regarded as primary; when Dr. Patterfon, with the advantages he poffeffes over that illuftrious character, and with cafes and diffections fo ftrong in point, came no nearer than putting caufe for effect.

“ Non omnia poffumus omnes.”

And here I muft advert to Dr. P.’s note, in which I am charged with oversight, in refpect to Sydenham’s theory of chorea, becaufe I fay, he “ fcarcely adverts to any thing but its curious gesticulations.” I confefs I did not think it neceffary to attempt a confutation of his theory, which afcribes it to an humour thrown on the nerves, &c. I had not overlooked it; but I muft leave it for Dr. P. to combat, unlefs indeed he means to uphold it; which mentioning it as he does, feems to imply. It is indeed, rather furprifing, that Dr. P. has not detected my theory in Dr. Sydenham’s; as mine depends upon the preffure of a fluid on the brain and nerves.

As to my being aware of the difficulty on this point, (Sydenham’s, &c. above,) and putting the queftion “ If hydrocephalus is the caufe of chorea, it may be afked, as the former is not an unfrequent difeafe, why the latter does not more often occur?” I confeffed myfelf ignorant of the reafon; but it by no means follows, as the Doctor fo confidently affirms, “ that it would *always* be produced, if it poffeffed the affinity of a fymptom.” I will only afk, if any diforder exifts, in which any individual fymptom of it, which may be named,—is not occasionally abfent?—Does not the Doctor himfelf allow, as well as others, that one of the moft leading fymptoms of hydrocephalus, *viz.* ftrabifmus, is not always prefent? Cold applied to various parts, will produce a diverfity of complaints, attended with their peculiar fymptoms: in like manner it may be imagin-

ed that pressure on different portions of the brain, may induce a diversity of symptoms. The objection of the Doctor to a thing devoid of certainty, does not prove, either that he is right, or that I am wrong. I must however, be permitted to enjoy my belief, that although hydrocephalus is not always, or even frequently, accompanied by chorea, yet wherever chorea does exist, there, hydrocephalus or its preceding state of inflammation and turgescence of the vessels, (which I presume may also be chronic,) also exists as the cause. It is not necessary I apprehend to its existence, that a gallon of water should be effused ; for the disease may be as complete with but a tea-spoonful, as with a larger quantity ; although it may be less dangerous, inasmuch as the brain will sooner accommodate itself to the pressure.

The Doctor asserts that in his second case of chorea, not a symptom of hydrocephalus can be traced, and adds, that my second case is of the same description. To this I may observe, that the assertion by no means proves that water did not exist in the brain. The Doctor however, gives reason to suppose, that some undue effusion, or determination of blood in the brain existed, when he tells us, that his patient “although addicted to much talking in his health, yet his loquacity is now greatly increased.”—And few will dispute the probability of the same in the case I have detailed, from the fatuity and idiotic appearance conspicuous in the patient.

“Instead (says Dr. P.) of being solely a symptom of chronic hydrocephalus, chorea sometimes proceeds from obstructed menses, sometimes from exposure to rigorous weather, but generally from irritation in the first passages.” I would ask, if the former of these supposed causes may not be itself, reasonably supposed dependant, on the disease of the brain?—or if it be the original source of the disease, whether it may not act by the

determination of blood, inducing effusion, &c. in the brain? In the same way, may rigorous weather, and irritation of the first passages promote chorea, by first inducing hydrocephalus.

As for convulsive agitations excited by particular irritations of the stomach; or choreatic gesticulations being attended by a spasmodic affection of the organs of deglutition; it would require more proof than the Doctor has brought, to convince me, that poisonous and other substances taken into the stomach, cannot excite determination to the brain, (as well as they can excite disorders of the skin,) and thereby be a remote cause of convulsions;—or, that the spasmodic affection of the organs of deglutition and the accompanying choreatic gesticulations, did not both depend on one common cause, viz. pressure on the brain.

The cases of Mr. Alexander, to which Dr. P. refers, are strongly corroborative of my sentiments respecting this disease, although Dr. P. has laboured hard to obviate the impression, which a review of the symptoms must necessarily create. In the first case, the symptom of strabismus occurred,—“a symptom (the Doctor says) of cerebral affection, yet hydrocephalus (he adds) shall not be the consequence.” No indeed! it shall not be the consequence, but was apparently the cause, both of it and of the chorea. The Doctor asserts that “the strabismus can be with reason ascribed only to the efforts of the brain to *recruit its impaired energy*, (*rifum teneatis!*) but cannot on good grounds, be imputed to hydrocephalus;”—thus does theory oppose reason!—The diminished tone of the alimentary canal, which the Doctor considers as the occasional cause of the convulsive motions, is certainly more rationally deduced from the pressure on the brain, to which it might then add additional force by the pressure of the contents of the alimentary canal on the descending aorta. “But surely, (adds the Doctor tri-



umphantly,) no theory, even the *newest*, can make us believe, whilst we are in our senses, (*Q. E. D.*) that every repetition of the strabismus was occasioned by the effusion of a fluid into some cavity of the brain, and that each cessation of the ocular distortion was the effect of the absorption of that fluid." I will venture to ask if there is any thing remarkably difficult in this belief? Do we not see fluids effused and partially re-absorbed, and again recurring, in other cavities? And why is the brain alone to be an exception? If this belief is even not correct, it is not unreasonable to believe that the strabismus ceased, when the brain had become habituated to the pressure excited by the fluid, and continued absent, until by a fresh addition of fluid, an inordinate degree of pressure was again excited.

The "morbid condition of the alimentary canal" in Mr. Alexander's second case, in my mind serves to strengthen the conviction, that the pressure on the brain was the cause of both it and the chorea:—not, as the Doctor supposes, that this morbid condition "in a constitution of considerable mobility," (an expression conveying no precise meaning,) is the general cause of the disease in question.

The third case, I view as equally conclusive on my side, (although Dr P. gives it against me,) more especially as it serves to evince the propriety of a different treatment in different states of the disease. For that "the tonic and antispasmodic system of prescription" is *invariably* just, will I think be advocated but by few. Sydenham bled and purged repeatedly in this disease; and the medicines employed by Mr. Alexander, were best adapted to the stage at which they were employed, though they probably might not have proved equally serviceable in the commencement. I presume Dr. P. himself will acquiesce in the justice of this observation, and that



he does not mean to say, that tonic and antispasmodic remedies are invariably proper.

The Doctor I observe, had some slight disposition to allow the doctrine to be just, (that chorea is a symptomatic disease,) when he adds, at the close of his remarks on Mr. Alexander's cases, that "they (the cases) all likewise tend to evince, that chorea is not a symptomatic ailment, *at least not naturally symptomatic of hydrocephalus*"—I must therefore believe that it has made more impression on the Doctor than he is willing to admit, or than his endeavours to overturn it, by his copious references, would seem to shew.\*

The case brought forward from the Medical Repository, is one which depends on a cause, originally so different from any other, that it is truly difficult to reason upon it. I cannot however perceive that it adds any strength to the Doctor's ideas respecting chorea, nor that it precludes the possibility of the existence of water in the brain, either previously existing, or excited by the primary source of the complaint itself.

If Dr. Whytt has shewn, as Dr. P. asserts, "that a delicate state of the first passages, or a depraved sensibility of their nerves not only disposes people to many complaints in those parts, but that the whole nervous system is thereby rendered more moveable, and liable to be affected by the slightest exciting causes;" may it not be equally inferred, that what is capable of

\* Both the symptoms and cure alike prove chorea to be *symptomatic* of pressure on the brain, and that it never is, nor never can be, an idiopathic disease. We might as well call the convulsive movements of hysteria, epilepsy, &c. idiopathic, without a reference to their primary source—of which those movements are merely symptomatic!

exciting such morbid action in the nervous, is equally capable of producing similar results in the arterial system, and thereby of promoting effusion in various cavities of the body; in which the brain would be likely to be a principal sufferer?

Is it possible the case of Dr. Macbride, adverted to by Dr. P. could not awaken in his mind one idea, that in a disease of 12 years standing, the brain was enabled so to accommodate itself to the changes induced in it, that no symptoms of disease should be apparent, except of symptomatic chorea? Where is the Doctor's morbid condition of the alimentary canal &c. &c. for so long a period? And why could not tonics and antispasmodics effect a cure? The answer is plain;—because the chorea was not idiopathic, and the remedies were not adapted to the original disease.

The cases adduced from the Bristol infirmary, are I think, strongly in my favour. The boy of 17, who received a blow on the head several years before, requiring the use of the trepan, although well, till within three months of the attack of chorea, was apparently during that period, slowly receiving the impression from watery effusion, although this chronic state of hydrocephalus did not produce the symptom of chorea till so long after. The brain by slow effusion, conformed to the increasing pressure; as is surprisingly evident in every part of the body: and could the cases enumerated, have been dissected, no one can dispute the great probability of finding the ventricles largely distended with water. In these instances, it can scarcely be called congestion; because the absorption of the brain, would correspond with the slow deposition of fluid.

The girl who died of hydrocephalus, after being cured by zinc, shews a case similar to mine related. Here, for a time

by the use of the zinc, chorea is suspended or cured; yet from some cause, an increased determination to the brain, (produced more suddenly than it had been accustomed to,) converts the chronic hydrocephalus into its acute form, and with a fatal issue.

The other girl is cured by bark and the cold bath; remedies, apparently highly proper at that period, to excite the absorption of the existing fluid. Some analogy might here be drawn from the conversion of chronic rheumatism into acute, if it was necessary; but every one can apply the arguments which might be used.

As for the case of Dr. Hales, in the note, of a woman receiving a severe blow of the head, &c. succeeded by chorea, during the existence of which, no symptoms of hydrocephalus appeared, and in which the cure was effected by stimulants principally;—I would ask if such a blow was not likely to lay the foundation for effusion as the cause of chorea; although the brain may have accommodated itself to the slow deposition, and thereby have been uninfluenced by symptoms, which its more sudden effusion must have induced?—especially as the Doctor allows that in a great majority of injuries to the head, no convulsions arise. And why? Because in general in injuries of the head, venesection, the best preventive of morbid effusion from arterial action, is timely had recourse to.

The five cases collected by Mr. Mullen, in the *Edinburgh Medical and Surgical Journal*, which were under the care of that able and judicious physician Dr. Hamilton, I regard as highly instructive; inasmuch as they afford sufficient evidence of the great importance of constant purging in the complaint, which, so far from debilitating, added strength to the system, by pro-

bably exciting absorption in the brain ; the influence of pressure on which, has a strong tendency to produce that apparently weakened but overburthened state of the system, (or what Dr. Brown has called indirect debility,) and which is to be removed, not by tonics and antispasmodics, but by depletion. It is a great pity Dr. Patterson has not stated what Mr. M'Mullin has said of Dr. Brown, as it is very applicable to Dr. P. himself. "Dr. Brown" says Mr. M'Mullin, "does not mention this disease particularly, but we can be at no loss for his sentiments with regard to it. With his usual dogmatism, he asserts that all spasmodic diseases, are diseases of debility, and yield readily to tonics and antispasmodics. The first part of this assertion is hypothetical, and the second is erroneous."—Here then we see Dr. P. has little to aid him, either in theory or practice, from the opinions of Mr. M'Mullen. And although I cannot agree with Mr. M'Mullen as to the cause of the complaint, I am truly rejoiced at the speedy triumph of the practice. And this the more so, because I see in its effects, the powerful, yet well known influence of evacuates, in promoting absorption of fluids in distant parts; and I see no one reason, on a careful perusal of the cases, to change my sentiments of the nature of the disease. Whatever may have been the cause of the black and fetid stools, there can be little doubt of the influence, which either their acrimony, or quantity, produced on the disease; for we find, that if at any time "the cathartics did not produce an evacuation, the involuntary motions recurred, and all the symptoms were aggravated." Now, that the suspension of the alvine discharge, should augment the disease by checking the absorption of the fluid in the brain, or by giving an opportunity to the exhalants to throw out an additional quantity, is not surprising; and rather adds weight to the theory I have advanced. Be this as it may, I think I may con-

fidently affirm, that if these cafes give no sanction to my sentiments, they afford still less to those of Dr. Patterfon.

The cafes adduced by Mr. M'Mullen, of chorea produced by teething, and on which so great stress is laid by Dr. Patterfon, do not, I apprehend militate against me. It is true I may not be able satisfactorily to explain the effect this irritation might have induced on the brain, disposed, it is possible, (nay probable, as we so seldom see a like effect from the cause assigned,) to hydrocephalus :—yet if Dr. P. will admit of analogy, I might mention the numerous cafes of tetanic convulsions excited by a slight puncture in a very distant part of the body ; and if such an effect may arise from a cause, apparently so trifling, it cannot be a matter of astonishment, that irritation on the gums may excite preternatural action in the vessels of the brain, and by causing effusion, induce a disposition to chorea ; which a removal of the cause, (by preventing the increase, or continuance of the effect, viz. effusion,) would immediately obviate :—One thing however, is proved from the cafes, viz. that however this irritation might act, the chorea induced, was symptomatic, and not a primary disease : and this is confirmed by the case mentioned by Mr. M'Mullen from Dr. Gregory ;—in which two successive attacks of chorea were removed, by taking out the first set of teeth, by the side of which the second set were pushing up. At the age of 15, this boy was again attacked with chorea, when *no cause* of irritation was discovered *in his mouth*, and he was now cured with extract of cinchona and sulphate of iron.—The important changes, which at the age of puberty take place in the system, may I conceive, be a sufficient cause of increased action in the vessels of the brain, disposed, by former attacks on that organ, to effusion ; which was removed, by the tone and vigor given to the system generally, enabling the vessels

of the brain in particular, to resist the continuance of undue determination to them,—or the absorbents to receive with equal facility, any fluid which the exhalants might pour into the ventricles.

That worms are very unjustly accused of exciting this disease by their irritation in the alimentary canal, I have endeavoured to shew in the essay which has called forth Dr. Patterson's remarks. That they may, when existing in a person affected with chorea, aggravate the disease, I have little doubt; but this must be by a primary effect induced on the vascular system, tending to augment the already overloaded vessels, or ventricles of the brain. The influence of purgatives in the cure of the cases related by Mr. McMullen, evince sufficiently, that when worms are in any case evacuated by purgative medicines, it is the operation of the cathartic—and not their mere expulsion, that benefits the patient; although, no doubt, much good may result from obviating a source of irritation, whether arising from worms or from a constipated state of the bowels.

Dr. P. has related the cases recorded by Sir George Baker, of certain poor children, “who by breathing vitiated air, were thrown into excessive tormina of the alimentary canal, which were attended with convulsions and delirium.” Surely, without referring this to the indirect irritation on the *prima via*, a nearer road might have led to the truth, when the *immediate* action of this vitiated air upon the lungs, is taken into view; which, by not supplying sufficient oxygen to sanguify, (if I may use the expression without the lash of Dr. P.'s criticism,) the blood; would produce such remora in the vessels of the brain, as may reasonably account for convulsions and delirium. Whether the tormina were produced by the vitiated air, or by, (as is

probable,) unwholesome food; it appears evident, that both they, and the convulsions were only symptomatic; and that the convulsions were less likely to be induced by the tormina, than by the cause I have assigned.

The three cases of chronic convulsions occurring in one family, as related by Dr. Armstrong in the ninth volume of the Edinburgh Medical Commentaries, and quoted by Dr. P.; are so very extraordinary, as to leave little room for reasoning upon them. I see however, small reason to connect them with chorea;—and as I have not undertaken to say, that *all* convulsive diseases depend on hydrocephalus, so I shall pass them over, without any further remark, than, that although no hydrocephalic symptom was apparent, it does not conclusively follow, that that disease did not exist. Dr. Armstrong considered them as truly epileptic, and I see no reason for dissent.

As for what Dr. P. says, respecting general convulsions attending an herpetic eruption, or epileptic fits from the stimulus of bile, in rheumatic fever, &c. I have nothing to remark, as his observations are irrelevant to the subject in dispute; inasmuch as he is speaking of convulsions generally; and I am only contending for *one form* of them; and it would too far extend the limits of my paper, to wander into another field for argument.

“In genuine hydrocephalus internus, (says Dr. P.) convulsive affections do not occur, *until the disease be far advanced, and the choreatic species of those affections is the rarest at any stage of it.*” I think, was I disposed to attack the Doctor unfairly, I might say, this is an approximation to my opinion. I will, however say, that it is the very thing I contend for. In



the acute stage, the symptoms are rapid, and readily distinguished; in the chronic, *gradatim*, and habit prevent the changes being so apparent.

“From a review, (adds Dr. P.) of 32 cases of genuine hydrocephalus internus, I find, that in 15 of them no convulsions took place: and that in any of the other 17 cases, spasms or convulsive motions did not occur, *until* the hydrocephalus had subsisted *several days*, nay in some, not until it had continued *several weeks*.” “Besides, from a review of a few cases of spurious hydrocephalus, wherein the head was considerably enlarged, it appears that, in some of these, the convulsions *did not* attack *at the beginning*, and that, in others, even in those ascribed to external injuries, no convulsions appeared.”

The last part of this paragraph, I add to prevent any idea of withholding evidence; though I have no where said, that either convulsions or chorea must inevitably follow the existence of hydrocephalus, but have distinctly asserted the contrary, without pretending to say, why it should so happen. As to the former part, I think it strongly conclusive in my favor; as the acute hydrocephalus, strictly speaking, is rarely a disease of weeks; and the Doctor allows that the convulsions did not come on, till *long after* the presence of hydrocephalus! What more could I desire, than this confession? for if the Doctor admits this; surely he might admit the same of chorea; which he above regards as a species of convulsions.

The cases adduced from Mr. Pott, to shew that in a majority of cases of wounds of the head, convulsive affections have not occurred, are entirely unconnected with our subject;—for until the Doctor shews, that by convulsive affections he particularly



means chorea, I cannot but think he is unnecessarily enlarging the original point of controversy. Yet even here I might safely meet him ;—for when particular information is given of the appearances on dissection of those who died, after convulsions had ensued, I think, without a reference to the cases, that suppuration, &c. producing pressure on the brain, was evident ; evincing the convulsions to be the effect, (and consequently symptomatic ;) unless the Doctor can afford *stronger* grounds for belief that the convulsions caused the suppuration, than he has that chorea produced the effusion, evinced in the dissection of his and my cases.

As for Dr. P.'s assertion, that my styled "*new theory* is nothing more than that pathological doctrine, known since the days of Hippocrates, and termed *conversions of diseases* ;" I can only say, I am sorry to see Dr. P. descend to subterfuges, to invalidate my claim, to what I consider a just view of the doctrine of chorea. I think it needless to follow the Doctor in this part of the dispute ; or to deny that any conversion of disease takes place in the complaint which is the subject of my remarks.\* I considered the Doctor as a more open champion for his cause than I find him to be ; and should therefore regard any triumph over him, (if the medical world should sanction my remarks,) as less considerable, than I should be otherwise disposed to consider it. I shall, however, observe, that the Doctor appears to have brought cases even here, to support my theory ;—as when he says "hydrocephalus internus has been transmuted into palsy, and palsy into hydrocephalus internus ;" and furnishes us with, what he says, is an apposite instance of the latter, from Dr. Ferriar. "*Eight months* after the appearance of the paralytic symptoms, the patient complained of severe head-ach ; vision became indistinct,

\* If it be as old as Hippocrates, why has Dr. P. endeavoured to give it to Dr. Percival ?

and at length was entirely lost. Epileptic fits *then* came on, and he died comatose. When the head was opened, the ventricles of the brain were found full of water, and several tumours, which in the prevailing medical language, might be called scrophulous, were observed in different parts of the brain." Here it seems to me, cause and effect are very evident, without recurring to transmutation. The scrophulous tumours (as the patient laboured under them elsewhere,) seem the original disease; and probably by their pressure induced the palsy. That they tended to excite the hydrocephalus, I have no doubt; nor that this hydrocephalus had been long advancing to its fatal termination; not as a chronic disease, but by some adventitious cause, evinced by the head-ach, &c. (the brain having accommodated itself by habit, to its previously diseased and deranged state,) which induced a sudden effusion, in addition, by which the symptoms of acute hydrocephalus became evident,—and epileptic fits ensued. This also shews that the epilepsy here, was a symptomatic affection and not a primary one.—It proves the connexion of those convulsive symptoms; and I will add, it strengthens the probable justice of my observations

Of the conversions of disease, I have no doubt; but I must be permitted to question it, in the present instance. I would add the same of the case related from Dr. Percival, of pulmonary consumption converted into hydrocephalus, by the violent succussions of coughing. That the coughing induced the effusion of water in the brain, I doubt not; but I can only consider the absence of the cough, &c. &c. after the symptoms of hydrocephalus ensued, as a suspension of the one, by the more violent action of the other. Still however, if I was to admit all the Doctor's fallacious reasoning from the various cases he adduces, I do not think it would add one particle of strength, to the weak fabric he has attempted to raise.

The case of Dr. P.'s patient, (neither "mutilated nor misconstrued by Dr. Coxe," as Dr. P. is pleased to affirm; but in which sentiment I trust no person will join him, after a candid perusal of the Doctor's own statement, and of the short abstract I have given of the case;) is not, I venture to affirm "a clear case of the conversion of chorea into hydrocephalus;"—but in my opinion, demonstrative of the truth of the theory I have ventured to bring forward. In detailing what I did of the case, it was not my intention to criticize either the Doctor's opinions or practice;—but merely to use those parts of it, which I imagined favourable to my sentiments. I shall here however, analyze more closely this case, in hopes of rendering those sentiments more plausible, than in my former essay;\* and I think I shall make it appear, that it was truly a case of symptomatic chorea, arising from the pressure of water in the brain, which the present explanation of Dr. P. serves but to confirm, although he affirms it to come under the second subdivision, third head of conversions, thus laid down by Dr. Ferriar. "If the original be a chronic disorder, such a state of the habit (favourable to the production of another disease,) may take place during its continuance, and the accessory disease may be simply super-added, or it may vary the form, or affect the duration of the former."

\* In this case (of a girl between eight and nine years of age,) the patient had for some years "been liable to *frequent*, tormenting head-achs, and *severe* stomach sickness; which were always removed at the time, by an *emetic*, whose operation pumped up some bile." A few weeks before Dr. Patterson's visit, (Ap. 3d, 1790) "an unsteadiness of her limbs" was first observed, with other symptoms of chorea. "She had a catarrhal complaint, attended with a *severe cough*, before the convulsive gesticulations were noticed." The Doctor found her pulse *slow* and small; and the eyes regular and sound. Bowels tardy, their habitual disposition. Head free of pain, and disturbance, &c.: a blister was applied between the shoulders; a flannel shift ordered; and the most acceptable diet to her palate.

The case I have first related "which (says Dr. P.) inspired him (Dr. C.) with the rudiments of the *new theory*, was a case of

I would here ask, whether the long continued head-ach to which she was subject, may not be imputable (if not to the deposition of water, at least) to an augmented determination of blood to the brain, at those periods of the accession of head ach? and whether the severe stomach sickness, and use of emetics, might not have been continually increasing the disposition to effusion, by still further adding to the determination of blood, in a part apparently in a weakened state? I would ask likewise, whether the severe cough before the convulsive gesticulations were observed, might not have produced the same effect, as is allowed to have occurred not however by conversion of disease, in Dr. Percival's case, as noticed in Dr. Patterson's observations with the view of shewing the influence of coughing upon the brain? Here then we have some room, at the very commencement of the history of the case, to apprehend the existence of the effusion, which Dr. Patterson supposes was the *effect* of the chorea; but which appears to me, more properly to be regarded as the effect of pressure, then existing.

It is by no means requisite to notice the daily prescriptions, which were chiefly of the tonic and antispasmodic kind, accompanied with occasional purgatives, exercise, the warm and cold bath, electricity, &c. I shall therefore observe, that on the 10th, after apparently mending, "an alarming faintishness came on, succeeded by languor, oppression, and much agitation," for which ether and laudanum were given. On the 22d, a little head-ach, for the fourth time since her present illness. She vomited twice on the 23d. On the second of May, she began with zinc and columbo. On the fourth, her hair was cut short, and her head dipped in the cold bath;—her appetite diminished, yet her strength was as good as before.

15th. Sickness and vomiting, &c. 21st. Removed into the country; in every respect seemed gaining ground. From this date to the 21st June, she was not seen by Dr. P. but regular accounts were transmitted to him.

24th. Fatigued by ride.—Very sick, reached, vomited, and threw up phlegm. 26th. Agitations morning and evening.

27th. So much agitated that the antispasmodic medicine was given along with the tonic. She has some little use of the *left*, but not any of the *right* hand.

ſimilar converſion," I can only ſay that this converſion,—(or perverſion,) exiſts only in the Doctor's imagination. It was this

28th. Very ſick.—Little or no uſe or power of the right hand ; the left ſhe can do almoſt any thing with.

I ſhall here beg permiſſion to inquire on what could depend the total loſs of power of the *right* ; whiſt the *left* hand remained capable of doing almoſt any thing ? Surely we ſhall not be told that the chorea was the cauſe ! I will aſk, what more reaſonable explanation can be given, than that preſſure, and that partial, on the brain, produced this effect ? and if preſſure, what more likely than that it aroſe from water, already exiſting in the ventricles ; which the diſſection but a ſhort time ſubſequentlly, proved to amount to three ounces ? It has been obſerved, that injuries ſuſtained on one ſide of the head, produce a paralysis of the oppoſite. Now as the diſſection ſhews that two ounces of fluid exiſted in the *left* ventricle, and only half that quantity in the *right*, may we not imagine it to induce this paralysis moſt particularly on the right-ſide ?

June 8th. Sick again.—I muſt obſerve that my reaſons, for mentioning the periods of her ſickneſs at ſtomach, which was accompanied with vomiting, is, that I conſider the ſtraining induced thereby, as adding greatly to the determination of blood to the brain, and conſequently rendering the diſpoſition to effuſion ſtill greater.

It is ſubſequentlly obſerved, that "notwithſtanding thoſe two ſick days, ſhe had got ſtrong and ſteady in the muſcular powers, ſo much ſo that ſhe was able to run about, to uſe her left hand nearly as well as ever, and to command her right hand in a viſibly progressive degree of improvement." This I ſhould apprehend, depended on ſome degree of abſorption of the effuſed fluid, by which the preſſure on the brain was diminished. This ſtate of improvement, was however but ſhort, for on the 13th ſhe had much head ach, and ſickneſs of ſtomach, which continued without relief three days ; and on the fourth, by domeſtic advice, ſhe got two grains of tartarized antimony, with camomile infuſion, which excited a watery evacuation by vomiting ;—her night's ſleep was worſe than uſual : appetite much impaired ; involuntary motion of the right hand ; and what ſhe looked at, appeared clouded, or rather ſtriped with red and purple.—Do not theſe ſymptoms indicate increaſed determination to the brain ? And what ſo likely to increaſe it, as the emetic ?

18th. Head and ſickneſs ſo bad, that a ſecond emetic was given, with the diſcharge of much bile and phlegm ; and ſeemingly with relief ; but the head ach next

case, and the dissection, which awakened in me a desire of investigating the subject, which till then, had not been

morning was equally bad. The bath was used, and being as ill as ever, she begged for more tartarized antimony, and went to bed very feverish. About nine that evening, three grains of James' powder were given, with no visible effect. She passed a tolerable night, but soon after waking, her head was as bad as previously.

20th. Four grains of the fever powder at eight in the morning, which proved *emetic*. Slept a good deal in the course of the day,\* yet complained much of her head; powder repeated at five and ten in the evening; a gentle perspiration of some hours. Head-ach, quick pulse and heat undiminished; the heart beat remarkably rapid; much flushed.—Could not water be yet suspected?

21st. Very little sleep; very hot, pulse quick, but not so strong as yesterday; sighs frequently; purgatives produced no effect.

22d. Dr. Patterfon saw her; and found her complaining *grievously* of the *head-ach*, fits of incoherent muttering, but in general sensible; face *flushed*, eyes composed, and regularly affected by light, &c.; pulse 90, hard and full.

From this particular attention to the state of the eyes, then, it is probable the Doctor now began to regard it, as a case of hydrocephalus, but not finding the symptoms he expected, he cannot yet view it as such; and we must conclude, his mind did not embrace the idea of water in the ventricles, until the coma and strabismus, &c. came on, on the 25th.

"Convulsive agitations vanished; and is possessed of as full power of her hands, &c. as consistent with her lately diminished strength." Blisters to the head, effervescent mixture and infusion of columbo freely given · emollient injections; two grains of calomel every night; and a few drops of thebaic tincture were added to two doses of the neutral mixture to compose the anxiety and restlessness.

\* In speaking of a case attended by Dr. Quin in 1771, (at p. 5. 6.) Dr. Patterfon says, "for we do not find the idea held by the parents, namely, that of the disease being a common fever, was contradicted by the Doctor, who accordingly permitted the administration of *James' powder*; the consequence of which was, *as might be expected*, that the stupor very soon came on." Now, can we suppose, that in Dr. Patterfon's case, the use of both James' powder and emetics, would be less likely to produce a similar result?

particularly the object of my attention. \* Will the Doctor affirm, that the chorea was converted into hydrocephalus,

23d. Rested tolerably; blister operated fully; much relief of head; pulse and other circumstances as yesterday; vomited soon after dressing the blister, &c. The Doctor returned home, and was informed by letter the next day, that soon after leaving her, "she was seized with a violent delirium, raving incessantly, &c." an emollient injection produced an evacuation, but no relief; she *lost her speech suddenly*, and groaned continually, &c. In some time a copious sweat broke out. It appeared she had suffered a paralytic stroke, as evinced by a distortion of the mouth, and a subsequent privation of her right hand. *Quere?* What did the previous privation of her right hand evince?

Here, I may observe, that the *chronic* hydrocephalus appears to be converted into the *acute* state, by some additional effusion, more rapidly made, than the brain could conform to; which I attribute to the action of the repeated emetics; for now, 25th, the Doctor found her in a *comatose* state; low delirium; *strabismus* towards the left side; *pupils dilated*, and *insensible* to light; flushed; breathing quick; *suspiria*; partial perspirations; convulsively affected in the *left* side; *torpid* in the *right* side; deglutition much impaired; pulse 110, and unequal; costive.

26th. Quiet during the night; but coma, &c. continued; *left* arm and hand more convulsed; *right* arm continues motionless.

This shews the affinity of the paralysis, and the convulsions; and that both were symptomatic, arising probably, from pressure on different portions of the brain, and differing in degree.

27th. General convulsions last night, between 11 and 12, recurring at different intervals, and of various strength until the last;—death closing the scene about two the ensuing morning.

#### DISSECTION.

Blood vessels of *dura mater* *turgid* and reddish, exhibiting strong marks of a preceding inflamed state. About two oz. of blood flowed from the longitudinal sinus by wounding it with the saw. Cortical and medullary substance of brain shewed no other morbid appearance than a kind of *serous diffusion*, over the convolutions of the lobes: the left lateral portion of the plexus choroides, seemed inflamed. From the left ventricle two ounces of watery fluid issued, and half that amount from the right. No other signs of disease observed.



when chorea existed to the last ; or that so large an accumulation of water as 12 oz. could have taken place suddenly, without producing instant death, when the slightest attention to the circumstances of the dissection must evince, that it must have been slowly accumulating ? The brain incapable of yielding, must have gradually accommodated itself by its absorption. I can now readily see the reason, why I did not then suspect hydrocephalus ; for I had no idea, that the chronic hydrocephalus could progress, with so few characteristics of its acute form. I was, indeed, then, in the dark ; and should truly have taken in "good part" the Doctors endeavours (however feeble,)

I have thus, without giving the minute detail of each day, stated the various circumstances which appear to lead inevitably to the conclusion I have drawn ; and I believe it is unnecessary to enlarge my remarks to a greater extent, as the whole of the essay will give my explanation of the various symptoms. After reading this case, I think few can doubt that the convulsions and the paralysis, were here the symptomatic effects of pressure from the water ; for it is not to be credited, that so much water could have been evacuated in so short a time, after the symptoms of hydrocephalus enumerated on the 25th, and the decease of the patient on the 27th. I must therefore, still believe that every circumstance upholds the truth of the opinion I have advanced, that the chorea, &c. were induced by the water which had been gradually accumulating, probably from the original commencement of her head-ach, many years before ; and that the effects of the emetics on the 17th and 18th of the month, were, by inducing too great a determination to the brain, to induce the inflammation apparent, and turgescence of the vessels, and thereby, also, probably to cause an additional effusion, inducing coma, delirium, strabismus and dilated pupils, &c.

I shall not follow the Doctor in the observations which the case has led him into ; except to say, that I think his case proves, that the "ancients and most of the moderns" were right in considering chorea under the general head of convulsions ; "although it be attended with several peculiar circumstances." I would ask, what but these "peculiar circumstances," could discriminate it from other species ? Cullen then, has not corrected an error, but has fallen into one, when he acknowledges in the later editions of his synopsis, that he was mistaken in supposing it to be merely a variety of those maladies. His stumbling from truth, into an error, is greatly to be regretted, since even the errors of so great a man are so readily embraced.



to furnish me “with some *light* upon the subject.” His case I thank him for, as it has done more than his arguments; but to be led by him, I should regard, (if I am wrong,) as the case of the blind leading the blind; and the event which would follow, the Doctor well knows; I hope therefore he will “take in good part” my leaving him to grope by himself; unless these remarks may aid him; as I conceive his doctrines require much further elucidation, than he has thrown upon them.

If the Doctor had read with candour my explanation, I think it impossible he would have thus expressed himself. “Very much in the dark, indeed, must he have been, and labouring under great perplexity, when we find him expressing his ideas in the following confused and contradictory terms.” ‘The *water* must have been accumulated, (*accumulating*, I apprehend it must have been in the original,) a *considerable time*, as evinced by the quantity, and the very enlarged state of the foramen ovale. Its *sudden effusion*, must have produced apoplexy; but the *slow progress* of the effusion permitted the brain to accommodate itself to the pressure.’—‘The *water in the brain*, was not I believe, the immediate cause of death. Some *sudden effusion* or congestion, was the source of the fatal issue, by producing apoplexy.’

Few persons will consider the first quotation of mine, given by the Doctor, as confused or contradictory; and I apprehend the deduction at the end, will be opposed by yet fewer; a due attention to the text and context, might have shewn the Doctor, that the last quotation does not contradict the former; for I apprehend, it will be allowed, that if the brain had accommodated itself to this great pressure, it could not be the immediate cause of death in itself; and yet that some sudden effusion or congestion in addition, above its capability of ac-

commodation, might very probably induce apoplexy and death.

I shall not say much on the illiberal and ungentlemanly attack on the "*medical treatment*," as it would favour too strongly of the *disposition* which led Dr. P. to employ his pen so *unworthily* upon a supposed intention to offend him. I shall merely say, that I candidly stated it (whether right or wrong;) as I considered it proper in investigating the subject, to withhold no circumstance, even if militating against me. I think however, that whoever reviews the immense proportion of fat in all parts, (as shewn by the dissection,) the inordinate appetite, and the enormous accumulation of blood in the vessels of the head, will see reason to dissent from the idea of Dr. P. of the patient being in a "reduced condition," although he had lost above 100 ounces of blood, (in above four months, a quantity by no means uncommon in as many days, on this side the Atlantic, and which might probably have *really* benefited the patient under consideration, if he had lost it in the same space of time.) If the Doctor has no more sense of the propriety of conduct due from one gentleman to another, and especially of that decorum, so essential in members of the same profession; I would at least advise him to review his own practice, and see if the unwarrantable application of the line addressed to me, may not reverberate upon himself.

As to "the practical mischief, which would ensue from embracing the *new theory* of Dr. Coxe;" on which Dr. P. says he "need not particularly enlarge, as it will not be difficult for any of your readers, (of the Medical and Physical Journal,) to perceive it, by impartially weighing the preceding facts and observations." I can only say, it is certain, if my view of the disease is correct, (which I firmly believe,) no mischief, but the reverse must result; and I shall merely repeat the observa-

tion, as applicable to the Doctor's sentiments : for although Doctor P. may think "there is a mischief to which that theory ministers, viz. the propensity to generalize without sufficient data, which requires some distinct animadversions ;" I must inform him, that he is not the arbiter of the opinions of others in medical science.—Sub judice ;—let the medical public decide : in their opinions I cheerfully acquiesce, though not in Dr. Patterson's animadversions, unless they carry greater weight than in the present instance.

I cheerfully accede to the observations which Dr. P. has quoted from Mr. McMullen, inasmuch as I conceive them very applicable to the Doctor ; and I would advise him seriously to revolve them in his mind, and endeavour to draw that instruction from them, which he seems so much to require.

With respect to the unhandsome remarks of my "trying every sophism, which my art of logic can furnish, such as the *petitio principii*, the *non causa pro causa*, and the *fallacia accidentis*, I feel totally unconscious of such proceeding ; and therefore conclude, that they could only proceed from a man, capable of acting in the manner he charges another.

Whether my inductions, from a careful review of the subject, shall be found just or false, is indeed a matter, as it respects myself, of very little importance or concern ; but regarding it as true, I view it as important in the enlarged field of medical science ; and I may say the same, of the arguments I have adduced in drawing the parallel between chorea and hydrocephalus, which Dr. P. considers as fallacious.

In opposition to the assertion Dr. P. so confidently makes, "nor is he more observant of the canons of pathology, than he

is of the laws of reasoning. According to his pathology, chorea is merely a symptom of hydrocephalus, and yet, in his parallel, he reckons that *single symptom* similar to the *whole disease*;" I conceive, a reference to the parallel of the two diseases, will shew my meaning to have been, that the *supposed* causes and symptoms of chorea, are also actually those of hydrocephalus; and hence, that a striking probability exists, of the latter exciting the former; by no means reckoning that single symptom as the whole disease; but, if a disease it is called, (and not a symptom, when it does occur,) that it is a symptomatic disease, depending on another for its existence. I apprehend, therefore, the Doctor will find it difficult to establish the point intended, if even he actually gives himself credit for sagacity in detecting it.

By this time, I hope the Doctor may know, (at least if he will take my word for it,) that no "great self-complacency" attended the "conferring on my arbitrary proposition, the title of a new theory;" further than every man may innocently enjoy, who really supposes the ideas he brings forward, may prove beneficial to that profession, of which he is a member. And although Dr. P. is so very facetious, as to discover a parallel for it, I hope he does not regard *himself* in the light of the *friend*, who being "*more conversant with books*" has convinced me, that my ideas have been anticipated. Such a friend I should surely esteem, if he had only been the means of sparing the Doctor's blushes, for his ungenerous attack upon me.

The ingenuity of Dr. P. must surely have been severely tried, when he denies the propriety of the word "new," as applied to my theory, because "a very old writer" has pronounced that "*there is no new thing under the sun.*" To the contents of the volume of which this saying forms a part, I

would recommend the Doctor to pay attention, as he may likewise find there, that "Charity thinketh no evil."

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Having now gone through my reply to Dr. P.'s polite communication; and endeavoured to vindicate myself from the charges brought against me, I trust I shall be believed, when I say, that my opposition to the Doctor's opinions, as expressed in his letters, was written with the most perfect good will and friendly disposition to that gentleman. Indeed, how could I intentionally wish, by my remarks to make an enemy of one, living in a distant part of the world, and known to me only by his writings? Surely common sense might have convinced the Doctor, that his suspicions were groundless; and perhaps the time which has elapsed since he penned his remarks, may have enabled him to see things in a less distorted point of view, and possibly also to think he has been both hasty and unjust in his measures. It was but a few days ago, that I knew such an attack had been made upon me; and this ignorance arose, from the unfortunate failure of the number of the Medical and Physical Journal containing it, reaching me. Although the Doctor is not disposed to give credit to my theory, I hope he will to my veracity, when I assure him I feel no ill will towards him; and had I not considered it a duty to reply to his call, "for refutation or concession," and at the same time to vindicate myself from his harsh insinuations, the present observations would not probably have been brought forward. As the general opinion I entertain of the subject in dispute, is pretty largely entered upon, in my former communication in the Medical Repository, I have not thought it necessary to strengthen by references, that opinion here; especially since it appears from what I have said, that I consider the Doctor as having done it to my hand, in those numerous cases

which he has brought forward, (a proof that he sometimes "peeps" into *new*, if not into old works,) all tending to uphold my ideas. I shall therefore, here close the dispute, by advising the Doctor seriously to consider how unjust he is, both to himself and his opponent, when he mistakes abuse for argument; and a *supposed* misrepresentation of his sentiments, for the deliberate censures of a person unknown to him.

JOHN REDMAN COXE.

PHILADELPHIA, *March 5th*, 1807.

*Extract of a Letter from DR. DANCER Physician in Kingston, Jamaica, to DR. COXE. Dated January 30th, 1807.*

ONE word more on the subject of contagion.\* The combatants for and against contagion, may I think, now come to a fair compromise. It is admitted† that though yellow fever is an endemic of local origin, taking its rise ordinarily from a vitiated state of the atmosphere, that it can also be propagated by *effluvia emanating from the bodies of those labouring under the disease*. Is not this the same thing as to say, that it is contagious like other diseases, ever hitherto deemed so, as typhus, &c.; taking the word contagious in the common

\* Dr Dancer being one of the warmest advocates for contagion, we give his observations a place with pleasure, in hopes that a liberal and temperate discussion of the subject, will at length determine the question to the satisfaction of both parties; and we do this more willingly, because it has been objected, that the discussions which have been allowed to appear, have been only on one side. Should this therefore lead to a reply, it is trusted that it will be free of asperity, or personal reflections.—*Editor.*

† See Dr. Firth's inaugural thesis on malignant fever, in which is summed up all the arguments that have been adduced, to prove the non-contagiousness of yellow fever.

acceptation, that is to say *communicable from one person to another*? That the yellow fever in Jamaica is at *least sometimes*, (though not always, or commonly,) propagated in this way, has been evinced by numerous and incontestible facts; whilst on the other hand, intermittents and common bilious remittents arising from marsh effluvia, particular winds, or certain states of the weather, however endemic or prevalent, no one had ever reason to think were communicable from one person to another.

The new definition of contagion, viz. that it is *the vitiated product of vascular action* may be considered as exceptionable; but *admitting* this part of it, when it is added, *that it is in all cases a specific secreted matter*, and that no truly contagious disease can occur oftener than once during life, much greater doubt will arise. How much better a definition is the old one, viz. *that contagion is that peculiar morbid poison, which is prepared in living animal bodies, and which is communicable either by contact or by near approach*. This definition, founded on no theory, will comprehend all the diseases termed contagious, however widely and essentially they differ from each other. Some of these are preceded by, or accompanied with febrile symptoms, as small-pox, measles, &c.; others are wholly without or with but slight fever, as cow-pock, &c. Some run a determinate course, others not. Some are communicable at greater or smaller distances, through the medium of the atmosphere, as measles, influenza, small-pox; others only by contact or inoculation, as cow-pox, lues venerea, hydrophobia, lepra and yaws; and others in both ways, or equally well, by means of the surrounding air, and by the contagious matter applied, as typhus, plague, small-pox, &c.

To corroborate the doctrine of non-contagion in the case of yellow fever, it has been strenuously denied that typhus or



even the plague is so. It is said, the plague is caused by noxious exhalations in certain places and during certain seasons, because the breaking out and termination of the disease correspond with the mutations of the year. How far this reasoning may apply to any particular countries as *e. g.* Egypt, where the unhealthy seasons may possibly give rise to the operation of contagion previously existing, I shall not pretend to determine : but it certainly will not apply to the several great plagues at Marseilles, Moscow, London, and elsewhere ; which were evidently *incidental*, occurring but once, or if oftener, at very distant periods of time ; and could not, therefore, be considered as originating either in local causes on the one hand, nor on the other, in any supposed occult quality of the whole region of the atmosphere, from planetary influence or meteorological causes ; because, then the plague would not be contagious amongst a few, or numbers ; but would affect all the inhabitants of that part of the globe at once ; and no means could be employed to check its progress contrary to what is known as a fact, viz. that the propagation of the plague can be restrained, by isolating those affected with it, and preventing any communication between the sick and well persons. The flourishing pen of a rhetorical writer can paint in ridiculous colours the universally received opinions of mankind ; opinions grounded on established facts, and the experience of all times, ancient and modern. Whether this gentleman wishes in every thing, to appear singular, or hopes to acquire celebrity by differing from all the rest of the world, I cannot say ; but bold and unqualified assertions will not go for proof, nor impose belief on thinking and discerning persons.\* I am satisfied that those who are unprejudiced, and who will not allow themselves to be confounded by a misapplication of terms, will consider the plague as a contagious disease ; and that many other fevers

\* See Dr. Mosely on plague and cow-pock.



analogous to the plague, are also contagious ; that is to say, are produced by effluvia or emanations from the sick, or from matters imbued with these. If this was not the case, to what purpose are the ablutions, the destruction of infected apparel, and all the other precautions employed for arresting typhus, and which have been so successful in Manchester, Liverpool, and since in London, in abridging the bills of mortality ? Whether the yellow fever of North America is one of this description, I cannot take upon myself to say ; but it seems to have been allowed by those, who nevertheless are opponents to contagion ; but this is quibbling about words.

I may perhaps be charged with not rightly understanding Dr. Ffirth and others, or of not rightly interpreting them.

They say, that diseases arising from the effluvia of sick bodies are *infectious*, not *contagious* ; but this is to confound terms : for such diseases as are received *directly* or *indirectly* from other sick persons, have ever hitherto been denominated *contagious*, in contra-distinction to others, depending on alterations in the atmosphere, or on unwholesome matters floating in it.

It is next affirmed that the exhalations arising from the bodies of persons in health who are in confined places, are equally noxious.—That they are noxious and capable of generating disease is well known ; but whether they are *equally* so, and always of the *same nature* as those arising from bodies under disease, has not been made to appear. That typhus can be thus produced, will be admitted, and being once produced, it will be afterwards contagious or communicable directly to others. The contagion generated will not produce any other fever or disease, but that of the persons from which it was received.

There can then be no objection to the calling such fevers contagious, except that they do not correspond with the new definition of contagious diseases, viz. that they depend on a specific matter, produced by secretion or living vascular action. But why may not some contagious diseases depend on matters of *excretion*, as dysentery evidently does? and why may not chemical combinations take place under certain varied conditions of the body, and produce a virus, capable of regenerating a disease of the same kind? Is not this the case in cancer?

I have no intention in the foregoing remarks, to challenge fresh disputation; nor would I be thought insensible to the merits of those, from whom I have the misfortune to differ. The thesis of Dr. Ffirth to which I have particularly referred, displays great ingenuity, and entitles him to a considerable degree of praise; but the compliments, with which he concludes to his *alma mater*, may be thought invidious. Whatever proficiency may have been made in the American schools, they have not yet eclipsed the seminaries of Europe; and it is a kind of treason in those who have derived all their acquirements at Edinburgh and London, to impress on the minds of their pupils, any sentiments to the prejudice of these places, or other universities; or to insinuate, that students have no where but in America the freedom of inquiry, but are forced, every where else, to listen to, and adopt implicitly, the dogmas inculcated by the professors.\*

\* We would suggest, however, that there is some cause for this insinuation. A reference to the seventh volume of the Medical and Chirurgical Review, p. 282. will shew, that a dissertation on intermittent fevers, by Dr. Baëta, was refused admission, "as a specimen for graduation, merely because it was a commentary on the doctrine of Dr. Darwin; the Dean of the Faculty of Medicine observing at the same time, that it would not fail to disgrace the university." *Editor.*

*Answer to DR. DEWEES' Reply to DR PEACHY HARRISON's Observations on Impregnation.*

*Harrisonburg, Rockingham, Virg. April 22d, 1807.*

SIR

YOU will be so obliging, it is confidently hoped, as to assign a place in your excellent Museum to the following answer to Dr. Dewees' reply. It claims your indulgence the more, perhaps, because it is the last time, that, on this subject at least, you will hear from your obliged and

Humble servant,

PEACHEY HARRISON.

DR. JOHN REDMAN COXE.

THE aversion of Dr. Dewees to controversy, I do assure him, cannot be greater than that which I feel; and he would do me justice to believe, that I have not entered the field from an expectation of reaping laurels, nor from a disposition to controvert, but for reasons much less deserving of censure, viz. that by a collision of opinions, some rays of light might, if possible, be cast on this obscure part of physiology. I am happy further to assure Dr. Dewees, that I greatly revere his talents, his learning, and his labours; and that I do not derive more instruction or pleasure, from the compositions of any writer in the Medical Journals of our country, than from his. And I intreat the Doctor, if ever he condescend to notice this answer, that he would not regard me, as one who wishes to provoke his resentment; but as one who would willingly sit at his feet, and receive from him those instructions which his well cultivated talents would enable him to give. But when we consider, that the greatest men are liable to mistake, (and in matters of speculation more so perhaps than others;) and that the most expanded and enlightened minds exhibit marks of imper-

fection; it cannot be regarded as presumption in me to believe, however I may revere his abilities, that the Doctor has embraced an hypothesis,\* with a view to explain the phenomena of impregnation and superfœtation, which he will not be able ultimately to support. And although, from the burlesque and ridicule, with which he has thought proper to treat my observations, he has conveyed an insinuation that it was presumptuous in me, to assert opinions different from his, yet I hope the Doctor will upon maturer deliberation, not only allow me to believe differently from himself, but even freely to declare that difference of belief. It is a natural right, and one which the greatest authority cannot invalidate or take away.

Before I begin my remarks upon the particular articles of the Doctor's reply, I beg leave further to assure him, that my convictions have not been altered by any arguments I have found in his reply; and that I do not persist in my opinions, contrary to the light of evidence, as it appears to me. And when I assert the existence of facts not generally received, I claim of him and the public only that degree of credit, to which an honest man would be entitled in a court of justice, who should depose to certain facts unsupported by correlative testimony. And I declare to Doctor Dewees that I had been revolving this subject in my mind for a year or two, before I saw his essay on superfœtation; and that I had ascertained, to my own satisfaction at least, the existence of certain facts, which, if confirmed by the observations of others, strongly militated, I was persuaded, against his hypothesis. These facts, it is true, I did not substantiate by the testimony of others, neither was there any attempt made to do it; and my reasons were, that I wished to give a concise view of *my own* sentiments on this subject; I did not regard the hypothesis as mine, it being held in

\* I say embraced, because I am able to shew that he is not the inventor of it.

substance by all physiologists so far as I knew; and particularly that the facts which I adduced, in support of what I believed to be the old hypothesis, were not *directly* maintained by any authority with which I was acquainted; I say directly, because indirect and collateral testimony is not wanting, and will be adduced in this answer.

In the ninth number of the Medical Museum, page 30, the Doctor begins his attack with a vein of humour, which shews, that he is capable of assuming the character of a wit when he pleases, as well as a philosopher; but as this will only make the sober reader smile, without producing conviction, we are but little concerned about it. But it seems to us, the Doctor has vainly spent his time, in criticising upon the phraseology of "extorting from nature her oracles," as he well knows that the same thing has been expressed a thousand times, if not in the same words, in words at least of the same import: and particularly that Virgil in his beautiful fable of Aristæus, to which I had immediate allusion when I used the phrase, has made use of a similar structure of language. To save the Doctor the trouble of turning over to it, I will cite the line which seems most to my purpose. In the fourth book of his Georgics, 398th line, the poet introduces the goddess's mother of Aristæus, speaking thus of the prophet Proteus.

Nam sine vi non ulla dabit præcepta——

The instruction intended to be conveyed in this place by the philosophical poet, was no doubt, that truth is generally discovered by diligent inquiry and persevering labour only. And similar to the above cited passage, I regard the following lines from Cowper's Task.

—— Will not God impart his light  
To them that ask it? 'tis his joy,  
His glory and his nature to impart.  
But to the proud——  
Or negligent inquirer not a spark.

Now if this be our meaning, (as it certainly is,) the Doctor's wit in the second paragraph of his reply, falls "fine ictu," without force; like the javelin hurled from the enfeebled arm of Priam.

But a charge is entered by the Doctor against my opinion, as being without foundation, and not even plausible. Why? Because the facts upon which they are professedly founded, have not been supported by the testimony of others. May I be permitted to inquire, upon what foundation his hypothesis rests?—upon facts? I declare, that after reading his essay upon superfoetation repeatedly, and with the utmost attention of which I am capable, I am unable to find any from which his hypothesis could directly be inferred: and he confessedly admits, that he takes those facts for granted upon which he builds his hypothesis. Oh! but he first proves, that superfoetation cannot take place according to another hypothesis, and therefore it must take place according to the hypothesis he has suggested! What kind of philosophy is this I beseech the Doctor? Has he learned this mode of reasoning from Lord Bacon, or from Sir Isaac Newton? I imagine not. Did he forget, that if one hypothesis were proved to be false, a hundred others equally erroneous and absurd, might be formed? One should expect, that he who refuses to another the privilege of reasoning from any other than "established facts," would take care not to depart from this rule himself. But has the Doctor restricted himself within these limits in his essay upon superfoetation? let the public judge. But he will perhaps reply, you profess to reason from "the responses of interrogated nature," and are therefore restricted to the limits which all sound philosophy prescribes, while I, "who boast no such familiarity with this high personage," am allowed to scamper in the wide field of conjecture and probability. Now the Doctor may be less cul-

pable than I, in not "professedly" reasoning from facts; otherwise I cannot see in what our cases materially differ, even according to the representation the Doctor himself gives of mine. I founded my opinions upon what I sincerely believed (and do still believe) to be facts; he upon what he conceived might be facts. I did not demonstrate the existence of the facts which I asserted: the Doctor confessed he could not demonstrate the existence of those, which he supposed. It is true there may be this other difference between us, that he may have a better right to speculate upon obscure subjects than I have; and he may be able to frame more plausible hypotheses. But who will be able to say, which of the two hypotheses is the truer, if both be built upon assumed principles; for it cannot be shown "were reasons, even as plenty as blackberries," that plausibility is any certain test of truth? This will not be construed as a concession, that the hypothesis which the Doctor has endeavoured to support, is at all more plausible than that upon which I have went.

But the Doctor asks, page 31, "is the gentleman certain that it was from nature's oracles, the responses came?" I answer, that as to what I consider the main fact, and what he himself acknowledges, if a fact at all, the most material one, viz. that venereal sensibility has its principal residence in the os tincæ; I have that degree of certainty which results from the concurrent testimony of experiments, repeated frequently, patiently, and perseveringly, and instituted with a view to ascertain this very point. But however reluctant, I am here compelled to acknowledge, that I am unable to support this fact by the direct testimony of any authorities to which I have access. But is it therefore certain, that it is not a fact? If I may be allowed to compare small things with great, was it in the power of



Harvey to substantiate the fact of the circulation of the blood, by the testimony of other writers? And if Jenner had been called upon to prove from public record, that the vaccine virus was a substitute for the variolous, was that in his power to do? But these are cases susceptible of being proved by demonstration or direct experiments; true,—and so I maintain the case before us to be. I appeal to the candid readers of the Medical Museum, whether it be not a fact, capable in its nature, of being satisfactorily ascertained? I ask, is it not capable of being ascertained, whether there be such a thing as venereal sensibility in the female genital system? And if so; in what part of that system it chiefly resides? Whether in the uterus or in the vagina? If in the latter, whether in its lining membrane generally, or in the os tincæ eminently? Are not these last mentioned parts at least, in the power of actual and direct experiment?

The reasoning of the Doctor in pages 32 and 33, appears to me unsound and illogical. The sum, so far as I am able to understand so perplexed a chain of propositions, is “that the os tincæ, from its general want of sensibility, and firmness of texture, is but ill calculated for being the seat of venereal sensibility; more especially as all women have this part; but there are very many women, (very many being in the superlative degree must mean a majority,) who feel no pleasure from the venereal congress, and others to whom this act is truly disgusting, and yet are prolific.” “Now,” proceeds the Doctor “unless it can be proven, that the os tincæ of the woman who feels no pleasure, and that of the one who does, be different, we must conclude, that the os tincæ is not the seat of venereal pleasure.”

Now if the Doctor means, by “very many” a majority of women, it will be very much doubted “whether this circum-



stance obtains." And indeed I cannot believe, that he meant to assert, that a majority of women are destitute of venereal sensibility : for this would be, in our conception, to contradict plain matter of fact, as manifested by the conduct of women in all ages of the world, in every condition of society, whether of the savage, the barbarous, or the civilized. But the Doctor's "very many," only means a few, when compared with all the women in the world ! Say then it is a fact, that a few women enjoy no pleasure in the venereal congress, how then will the Doctor's argument stand ? thus,—a few women, who have the *os tincæ*, and who are prolific as well as others, feel no pleasure in the venereal congress ; therefore the venereal sensibility does not reside in the *os tincæ*. I leave it to logicians to decide, whether this inference is properly made from the foregoing propositions ; and whether this reasoning, if it conclude against any thing, does not more logically conclude against the necessity of pleasure in the prolific intercourse, than against the seat of the venereal sensibility being in the *os tincæ*. If there are indeed women who, when they are in a situation to be impregnated, feel no pleasure in the sexual intercourse, and are yet prolific, I grant that this proves that pleasure is not an indispensable requisite in *foecundation*. But this argument no more disproves the principal residence of the venereal sensibility in the *os tincæ*, than it disproves there is such a principle as venereal sensibility at all : for suppose, this principle to reside any where else in the genital system ; have not all women this part too ? Now, then I retort, "unless it can be proven that those women who enjoy pleasure in the venereal congress, and those who do not, have an organic difference in the part in which the venereal sensibility resides, we must conclude that **THIS PART** is not the seat of venereal sensibility." Thus this argument of Doctor Dewees, by proving too much, proves itself to be false.

But let us examine some of the propositions contained in this complex argument, separately ; and see whether it may be rendered doubtful, at least, whether they are altogether to be relied upon. The first is, “the os tincæ from the firmness of its texture, seems but ill calculated for being the actual source of venereal pleasure.” But I would beg leave to suggest the inquiry, whether its texture be firmer than that of the seat of the sense of hearing, or of the sense of smell ? and whether the operations of these senses do not require as acute sensibility in order to receive the impressions of their proper objects, as the organ of generation ?

Dr. Dewees observes, in his essay on superfœtation, Medical Museum, page 169, that “no one has ever developed the muscular fibres of the uterus, yet the phenomena of labour puts (*put*) it out of all doubt that it possesses them.” Now it seems to us, that the same phenomena of labour which remove all doubt, with regard to the muscularity of the uterus, will justly entitle us to draw the same inference, with regard to it cervix. Is the uterus capable of being dilated and enlarged ? so is the cervix. Is the uterus capable of contraction ? so is the cervix. Now if the uterus be in any degree muscular, the os tincæ in which it terminates, is hardly so firm in its texture as to be incapable of being the seat of venereal sensibility. It is scarcely more firm in its texture than the snout of the hog, or the extreme point of the elephant’s proboscis ; and there can be no doubt but these possess the sense of feeling in a high degree. To these observations it is hardly worth while to add, that its texture is certainly less firm than that of our teeth, which are as sensible to the variations of heat, perhaps, as any other part of our bodies.

The next proposition into the truth of which we shall examine, is, that “the os tincæ, from its general want of sensibility,

seems but ill calculated for being the active source of venereal pleasure." But is not the term *sensibility* in this proposition ambiguous and equivocal? Does the Doctor mean by it, the abstract principle, or that specific sensibility which has been called *venereal*; or does he mean that, which is fitted to receive the impressions of tangible objects? From the member of the proposition, which until now I intentionally omitted, "as far as can be determined by the sense of touch,"\* one would be led to conclude, the last was meant. But suppose the term to be used in either sense, it appears to us not absolutely true. By sensibility, I understand that principle of our constitution, by which we are susceptible of pleasure or pain, from the impressions of external objects, or of internal operations. But will the Doctor deny to the *os tincæ*, the capacity of being the seat of *painful* sensations, as well as of pleasureable sensations? I think not. Now if there be in it a "general want of sensibility," it ought to be incapable of pain as well as pleasure; but this the Doctor will not assert. He will allow, I am persuaded, that it might be susceptible of *extreme* pain from any mechanical injury, say forcible dilatation. Now it appears to me an incontrovertible truth, that whatever part of our bodies is capable of being pained, may also be the seat of pleasureable sensations: but unquestionably, if a part is capable of being pained, it cannot be said to want sensibility. I will allow that the *os tincæ* may be freely felt by the finger, without exciting much of either pleasure or pain. But does it follow consequently, that the friction of the penis in coition, does not excite pleasureable, and perhaps in some, for aught I know,

\* That the expression "sense of touch," as used in this place, is not an Anglicism might be easily shewn; but whether it be an Irish-ism or some other ism, I will not undertake to say. From the construction of this sentence, one would be led to suppose, that SENSIBILITY is a tangible substance, in the opinion of Dr. Dewees.

painful sensations ? I need not undertake to prove to the Doctor, that there is such a thing as specific sensibility, since he has done that in his essay upon superfoetation, better than it would be in my power to do ; as also, that specific sensibilities are only aroused into activity, by their appropriate objects.

What wonder then, if Doctor Dewees did not discover venereal sensibility in the *os tincæ*, merely by touching ; especially when that was not done with a view, perhaps, to the ascertaining of this fact ! Now did the Doctor, while he was touching, find this principle in any other part of the vagina ? This would be an important article of information : for if he did not, the very same argument would prove the non-existence of that principle. If it be not in the *os tincæ*, or some other part with which the penis comes in contact, in the sexual commerce, it inevitably follows, it has no existence.

“ Besides” continues Doctor Dewees, “ women in the latter months of pregnancy, who do feel pleasure from sexual intercourse, have equal enjoyment when the *os tincæ* is entirely obliterated, or out of reach of the penis.” Upon this argument he seems to rely with great confidence ; and indeed, if the facts upon which it rests were established, it would be intitled to great weight. But of this I entertain great doubts ; for I have always understood it to be a general fact, that venereal sensibility is diminished during pregnancy, and that women are less disposed to admit the embraces of the male, during this period, than at other times. There may be particular exceptions to this general fact ; but such exceptions will not be allowed to invalidate the general fact. I would not, however, be understood, as denying that women may have considerable enjoyment from sexual intercourse ; this is perfectly agreeable to my own observations ; and if, as Dr. Monro asserts

it be "constantly tender" during pregnancy, is what might be reasonably expected.

But is it a fact, that the os tincæ even in the latter months of pregnancy, gets "entirely out of the reach of the penis?" I should suppose not. Is it not in every stage of gestation conveniently touched by the finger? If this is generally the case, and I am sure it is in some women, it cannot be "entirely out of reach of the penis." The following quotation from Dr. Monro's anatomy, places this subject, it seems to me, in precisely the proper point of view. "Moreover, says the Doctor, the cervex or neck of the womb itself, which has long remained unchanged, becomes much shorter during the last months of pregnancy, and at length forms a broad flat opening, which, toward the time of parturition, grows continually wider." This I take to be the true state of the case; but can it be inferred from this passage, that the os tincæ, even in the latter months of pregnancy, is "entirely out of reach of the penis?" Surely not.

Now if the os tincæ is within reach of the penis through every stage of gestation, and I think it would not be easy to evince the contrary, the Doctor's argument will be found to have but little force. If the cause we have espoused, were assailed with arguments; no more formidable in themselves than the one under consideration, no great apprehensions of its ruin need be entertained. Indeed it seems astonishing that Dr. Dewees should have asserted it to be a general fact, that women feel as much enjoyment in the sexual intercourse, during pregnancy as at other times. This assertion is certainly contradicted, by the experience of all whose marriage has been fruitful. It is pointedly and unequivocally contradicted by analogy; all brute females having an irreconcilable aversion,

during gestation, to the embraces of their males. In these, impregnation seems completely to suspend, for a time, the venereal sensibility. It seems equally surprising that the Doctor should assert it to be a fact, that the os tincæ, in the latter months of pregnancy, gets "entirely out of reach of the penis." Surely the Doctor has always been able to reach it with his finger, when he has made the attempt. And still, that he might seem to strike a decisive blow at my reasonings, it must be placed "entirely out of reach of the penis."

I beg leave here to observe, that I have no where said, that venereal sensibility has exclusive residence in the os tincæ; but on the contrary, whenever I have had occasion to speak of this subject, I have constantly guarded the meaning by the epithets principal or chief: thereby meaning to be understood, that other parts are endowed, in my opinion, with venereal sensibility; but *that*, more eminently than others. Consequently, if it could be proven, that the os tincæ in the latter months of pregnancy, is out of reach of the penis, a considerable degree of venereal sensation might still be accounted for, on the principles for which we have contended.

"We would be glad, says the Doctor, page 33, to know what is to be understood, by the female feeling an unknown desire." Without attempting to vindicate the use of the phrase, and without contending that it is a purely English idiom, I beg leave to refer the Doctor for the meaning of it, to the 412th line of the first book of Virgil's Georgics.

*Nescio quâ præter solitum, dulcedine læti.*

And to the 90th page of the Philadelphia edition of *Telemachus*, where he will find the following passage, "but as soon

as one trusted his caresses, one felt I KNOW NOT WHAT of poison." The Doctor's refined taste, and deep erudition in classic literature, will enable him to relish, as well as to understand the smooth and polished language of these elegant writers, much better than the blunt expression "unknown desire," which I was so unhappy as to use, for the sake of conciseness, in place of it.

He again asserts, page 34, that the *os tincæ* has little or no sensibility. Now suppose I were to assert, that the ends of his fingers, or the apex of his tongue, possessed little or no sensibility, or "certainly not that degree of it," as would render THEM the seat of the sense of feeling, because they are not painful upon the slightest impressions? What would Dr. Dewees, nay, what would every physiologist reply? Certainly that the ends of the fingers and the apex of the tongue, do possess the sense of feeling in a high degree, while painful sensations are excited in them, by violent impressions only. The inference I would have drawn is too obvious to be mentioned.

But the Doctor further asserts, that "the *os tincæ* having no fixed place in the pelvis, is by no means well situated to receive the reiterated frictions of the penis." I would be glad to know how the Doctor would have it placed, in order to be better situated for this purpose, than it really is. Suppose its situation within the PELVIS is quite uncertain, is it therefore uncertain whether it is within the VAGINA? No sir, and in the venereal congress, the penis, I suppose, has no concern any where else. Now I cannot for my life see any difference it can make, what situation it has in the vagina, in order that it may receive the irritation of the penis; for this will be unavoidable, what situation soever it may occupy. I would be glad to be informed why, if it were not for this purpose, the



cervex uteri was projected into the vagina. I solemnly declare, that I know of no other so good reason.

If the truth of the observations we have just considered, be very questionable, as it is conceived we have shown, the inference drawn in page 35, from them, viz. that the os tincæ "cannot from its extreme sensibility, be the immediate cause of the venereal orgasm," will be entitled to that weight before the public, by which its premises shall appear true. But if the Doctor will be so good, as to cast his eyes again over the paragraph he has quoted from my observations, page 423, he will perceive, I have not said that the "extreme sensibility of the os tincæ is the immediate cause of the venereal orgasm;" for this I think would be downright nonsense; and the Doctor by introducing as he does, "extreme sensibility" into the above noticed inference, makes me to speak nonsense. Irritation applied to sensibility, might be a cause of sensation; but sensibility whether extreme or otherwise, cannot, properly speaking, be a cause at all; a cause implying a power to produce a change in a subject, but no such power is implied in the term sensibility. With this correction, the Doctor's reasoning will stand thus; the os tincæ has little or no sensibility, its situation in the vagina is uncertain, and therefore irritation applied to it by the penis, cannot be the immediate cause of the venereal orgasm! Now if there be any connection between the premises and conclusion in this argument, I am free to confess, that I want capacity to perceive that connection; and how forcible soever the argument might be, it is certainly managed in a very negligent manner.

"But," continues the Doctor, "to make the seat of venereal pleasure resident in the os tincæ, is an assumed principle, which can neither be proved by analogy, nor experiment." And



that practitioners of midwifery, though they have touched it a hundred and a hundred times, have never discovered it to possess "extreme sensibility". If that be an assumed principle, I can assure the Doctor it was not assumed hastily. The discovery of it, (if indeed it be one) was accidental. I became convinced of the fact, while I was making some inquiries into the causes of barrenness. But why may it not be determined by experiment? Might not females for instance, if their attentions were directed to this point, be able to decide in what part the greatest pleasure is felt? and particularly, if when the venereal desire is aroused, gentle irritation be applied to the os tincæ by the finger, and if this experiment be frequently repeated, might not the female, at length be able to say, whether that was the principle seat of pleasure in the sexual embrace. To me at least it seems, that patient trials of this kind, might determine the fact. But as in the philosophy of mind, every one who would become skilled in that science, must attend for himself, to those intellectual operations about which it treats; so in the case before us, every one who wishes to be satisfied, ought to inquire for himself, whether the facts alleged be real or not.

But was the "touching repeated a hundred and a hundred times," *undertaken* with a view to ascertain this fact? I imagine not. I hardly suppose the practitioner had this point in view; and the patient, it is more than probable, was in no humour, during that operation, of attending to venereal pleasure.

The argument drawn from the conical form, and the smallness of the apex of the neck of the uterus, appears to us not very forcible. In truth we cannot see, why an organ of that form, and having so small a point, might not be the seat of sensibility, as well as any other shape. We know that the tongue

may be brought to a conical form, and its apex to as small a point as that of the cervex uteri, and yet possess the sense of feeling in a very acute degree. Neither can I see the force of the reasoning in the subsequent part of this paragraph. I shall not therefore, undertake to examine it in detail; but will pass it over with this observation, that the cervex uteri being "pendulous in the vagina," must of necessity receive the friction of the penis in the venereal congress: and that if it assumes a direction favourable for apposition, it is only during the venereal orgasm, at which time, according to Dr. Monro, "a convulsive constriction of all the parts of the vagina takes place,"\* by which the "projection of the penis" will be prevented from "rudely forcing the uterus upwards."

In page 36, we are told that "by venereal orgasm we must understand a SOMETHING produced by, or the consequence of pleasure:"—this is another instance of the negligent manner in which the Doctor has written his reply. He knew too well the superiority of his own abilities, over those of the writer of "Observations," &c.; this betrayed him sometimes into unbecoming levity, and sometimes into blameable negligence. Who in the name of common sense, would be the better informed by this definition, what is to be understood by venereal orgasm? But waving criticism, as to the logical accuracy of this definition, I beg leave to inquire whether the Doctor gave this definition as his own, or as one that he would wish to be considered as conveying my meaning? If he intends it as an exposition of what HE means by the expression venereal orgasm, how can I be justly chargeable with any absurd consequences which the Doctor may make to flow from it? If on the contrary, he meant by it to represent my meaning of the expression, it became him, as a fair reasoner, to shew, that I had any where asserted, that venereal orgasm was the conse-

\* See Monro's Anatomy, page 39.

quence of pleasure. This he could not have done. I have indeed said, that the venereal orgasm "is accompanied with exquisite sensations." That these sensations, are, in a majority of women pleasurable, I have not only the common sentiment of mankind, but the authority of Dr. Monro\* for asserting: and if I were to maintain that in some women, these sensations are of a disgusting or painful kind, the authority of Dr. Dewees would bear me out. But that some women are indifferent in a PROLIFIC congress, I am not at liberty to believe. That many women are *often* indifferent to the embraces of the males, I entertain no doubt; but to the opinion, that these are always so, I cannot by any means subscribe.

Although we are not bound to maintain that pleasure takes place in every prolific congress, yet we beg leave to notice the proofs against this doctrine, which the Doctor has adduced from the Abbé Spallanzani. The first is taken from the bitch; in which case, impregnation was effected by means of a syringe; but was this done at a time when the bitch was indifferent, or averse to the embraces of the male? No.—Spallanzani was too well enlightened on this subject, to expect he could effect impregnation at any other time, than when the bitch was *in heat*. But is it certain, that the stimulus of the injected semen produced no pleasurable sensations, especially when the genital organs were in a condition to receive with facility the impression of that stimulus? Dr. Dewees is inclined to believe, that women might also be impregnated by means of a syringe. But I must beg leave to think differently from him on this point, and for the following considerations: the bitch, and all other animals which feel the venereal impulse at certain seasons only, and have an aversion at all other times, can only be impregnated at these particular seasons. This

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\* See Monro's Anatomy, 3d vol. p. 29.

is a necessary constitution of their nature; because, being destitute of the rational faculties, if they never felt the venereal impulse, they would not use those means instituted by nature for the continuation of their species, and of consequence it would become extinct; and on the other hand, if they always felt this restless appetite without the counterpoise of rational and moral considerations, the greatest disadvantages and inconveniences would result both to themselves and to mankind. Such in general is the imperious demands of this appetite in the brutal kind, during the venereal orgasm, that all the other calls of nature, are almost entirely neglected. This predominance of the venereal impulse, cannot be more elegantly and forcibly described than in the following lines of the Mantuan bard.\*

Nonne vides, ut tota tremor pertentet equorum  
 Corpora, si tantum notas odor attulit auras?  
 Ac neq; eos jam fræna virûm, neq; verbera sæva,  
 Non scopuli, rupesque cavæ, atq; objecta retardant  
 Flumina, correptos undâ torquentia montes.

Such indeed is the avidity with which the male semen is received at such periods, that impregnation is for the most part, much more prompt and certain in brutal, than in human females; because in these the venereal orgasm is not subject to periodical recurrence; and the reason seems to be this, that the human species being endowed with intelligence, and made capable of feeling and obeying the restraints of morality and religion, it was therefore very proper, that the venereal appetite should be in them, less subject to ebbs and flows, so to speak, than in brutes; and that it should prompt more constantly to indulgence, but with an impulse not so irresistible. Its demands are never so imperious, but that they may be made to yield to the restraints of virtuous modesty. They will at all times ad-

mit the embraces of the males, but they are not subject to such violent paroxysms of this appetite as brute females; in the latter, the impulse of this appetite, when awake cannot be resisted, in the former it may be restrained, and its most urgent calls silenced, except when aroused by the irritation of the male organs, it then imperiously demands gratification. It is then easy to conceive, that brutes in which the venereal orgasm takes place, without the assistance of the male organs, may be impregnated merely by injecting the semen into the vagina; but the case is very different with regard to women whose genital organs we have shewn, are prepared for the reception of the seminal stimulus, by the irritation of the male organs only. These then, are the principal reasons why I do not incline to believe, that women may be impregnated by means of a syringe.

But that pleasure accompanies a prolific intercourse of the sexes, the Doctor attempts farther to disprove from Spallanzani's account of the manner in which newts and frogs procreate; because fœcundation takes place exterior to the body, he concludes, that pleasure hardly accompanied this process: I have not the work itself of Spallanzani, but in Duncan's account of it, which I have, it is stated that the "embraces of the male begin, before the exclusion of the eggs commences: and that during the discharge of eggs, the agitation and croaking both of the male and *female* were very remarkable." It is fairly deducible then, from this testimony of Spallanzani, that sensations of some kind, and most probably of the pleasurable kind, accompany the copulations even of frogs; and if so, the inference, Dr. Dewees would wish to be made from this case, is not warantable.

But waving any further examination of particulars in this paragraph, it will be sufficient for me to observe generally, that

it would not avail the Doctor any thing, to succeed in proving that the venereal orgasm is not the consequence of pleasure, unless he could shew *that* had been asserted by the writer of "Observations," &c. He has made the *venereal orgasm* essential in this process: but he has no where asserted that this consists in pleasure, or that it is the consequence of it. It is a very convenient and easy mode of contention to set up an assertion never made by our antagonist, but in so artful a manner as to induce the belief of its being his, and then argue it down with an air of triumph.

In page 37, the Doctor asks very superciliously, how I know that "the cervix uteri becomes turgid" in the venereal orgasm? I answer that I have been convinced of it by my own inquiries; but as my convictions will not be testimony with Dr. Dewees, I am happy to find them supported by authority no less respectable than that of Dr. Monro. "In like manner, (says the Doctor,) as in the male, the attrition of the very *sensible* and *tender* parts, excites a convulsive constriction of all the parts of the vagina. By these means the *return of the venous blood being suppressed*, the clitoris, especially in salacious women, grows *turgid* and erect, the nymphæ on each side swell as well as the venal plexus, which almost surrounds the whole vagina, and the pleasure is increased to the highest pitch," &c. "But, continues the Doctor, the same action which, by increasing the pleasure to the highest degree, causes a greater conflux of blood to the whole genital system of the female" &c. and again he asserts "the uterus itself (and will it be said that its cervix is not) is *now turgid* with influent blood."

But Dr. Dewees again asks "what proof have you that the ligamenta rotunda contract, during the venereal orgasm?" I

answer that my meaning was, that the os tincæ is brought nearer to the os externum, at the time when the genital system is in the condition described by Dr. Monro as the effect of the attrition of its very sensible and tender parts by the male organs. Of this fact, whosoever will take the pains to enquire for himself, the truth will be obvious. Supposing I was mistaken, in assigning as a means conducive to this end, the contraction of the ligamenta rotunda, of what great fatality to my opinions could this mistake be? None at all, if the fact be real. Candour obliges me to acknowledge, that the contraction of the ligamenta rotunda, without the co-operation of other causes, would produce the consequences the Doctor describes. But the co-operation of other powers, so as to depress the uterus in the pelvis, and approximate the os tincæ to the orificium externum is not difficult to conceive.

That the hypothesis which I have embraced is free of all difficulties I am not disposed to maintain. And I am ready to acknowledge, that the difficulties objected to it in the next paragraph of the Doctor's reply, are (the drollery apart) serious ones. But I really do not see how the deduction of a conclusion so universal, from facts, the fewness of which renders them almost doubtful, can be justified. Would the following be regarded as good logic? There are a few extraordinary cases in which impregnation is said to have been effected, without the introduction of the penis into the vagina; these cases, therefore "furnish most unequivocally the following conclusion," the introduction of the penis into the vagina in order to impregnation, was a useless and unnecessary provision of nature: and yet this conclusion, it seems to me, is as legitimately drawn as the other.

And here I close my answer to the first part of Dr. Dewees' reply; and am happy that I neither feel it to be my duty nor



my inclination to follow him in the scamper he has taken in the latter part of it, through the wild and romantic field of burlesque and drollery. On this part I shall only observe that my conjectures as to the *manner* of superfœtation, are liable to as few objections as his own, whenever he shall establish the *FACT* of superfœtation. "If he will not be able to do this, it is not my fault."

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*History of some Anomalous Cases of Disease.* By DR. TIMOTHY HALL.

*East Hartford, (Con.) April, 1807.*

SIR,

**I**F you shall think the following cases, or any one of them, worthy a place in your Medical Museum, they are at your disposal.

I am, Sir,

Your obedient humble servant,

TIMOTHY HALL.

DR. JOHN REDMAN COXE.

#### CASE I. Of Mr. G. W.

Mr. W. was out of health in a degree, for some years previous to the time in which the disease now to be mentioned, made its attack.



He was naturally inclined to *obesity*, led a sedentary life, indulged freely in eating and drinking, was a taylor by trade, and aged 45 years. His first complaints, such as shortness of breath, snoring in sleep, &c. were supposed to arise from the above mentioned state of his system and habits, and no particular attention was paid to them.

Two or three years previous to the commencement of his disease he supposed he had something of the asthma, and took some remedies for it, but with what success I do not know, as I did not prescribe for him. Sometime in the early part of the spring of 1805, he found himself gradually growing worse, which continued increasing till the 30th of April of the same spring, when he sent for me. I found him greatly distressed, attended with vast difficulty of respiration, considerable pain about the chest, could not lie in a horizontal posture, and had been sleepless two or three nights.

He informed me that if he got to sleep for a moment, in any position whatever, he was in danger of suffocation, and awoke with a sudden start. He also informed me that he had been subject to a good deal of uneasiness about the region of the *liver*, which obliged him to lie for some months past, constantly on one side.

He did not appear to have any thing of peripneumony, as almost all the pathognomonic symptoms of that disease were wanting. His distress, however, was so great, that a vein was opened, and as he found relief from bleeding, it was continued till about 24 ounces were taken; I then gave a purge, and in the afternoon, as his pain and distress had returned, the bleeding was repeated to about the same quantity as before.

May 1st. Repeated the bleeding again to about 24 ounces, and with evident good effect; then gave a purge, and antimonials as alteratives.

May 2d. Repeated the bleeding in a less quantity, as his distress had abated in a degree. After the last bleeding he was considerably easier, and soon became comfortable and continued so till the 5th, when his symptoms returned with great violence; gave calomel and squills, each six grains, in divided doses at short intervals, which procured a large alvine discharge and afforded great relief.

May 6. Dr. C. saw him and advised a continuation of the calomel and squills in doses of one grain of the former to three of the latter, formed into pills, two or three of which to be taken daily; this he continued till he had taken three dozen of the pills, with manifest advantage. Soon after finishing the pills he went among his friends in Glastenbury, and I did not see him again till the latter part of September following. He then returned here and was not so well; and as there had been some suspicions of hydrothorax from the commencement of his disease, and as the symptoms now more particularly seemed to indicate it, by paucity of urine being added to those above mentioned, which had returned in a considerable degree, I gave calomel and squills intending to bring on ptyalism; this afforded great relief as the mouth became sore and the spitting increased. He soon became very comfortable, active and lively, had a good appetite, ate *very* heartily, regained his strength in a considerable degree, and attended to his usual occupation for a few weeks: but now, apparently, from over-eating and want of exercise, became quite unwell again; his symptoms, however, were different from those he had before.

About the 20th of November, while I was absent on a journey, he was taken suddenly much more unwell; took a purge,

and in the course of the following night had a paralytic affection of his tongue which rendered his speech almost unintelligible. The tongue appeared thicker on one side than the other and no other part of the body was sensibly affected. I saw him three or four days after, bled him, gave a purge, applied an epispastic to one side of his neck, directed stimulating gargles, and his speech soon mended, but continued slow and hesitating for some weeks. The blood was considerably fizy.

Not long after this he complained again of his breast, made but little water, &c. He then took tinct. digital. (saturated) in doses of 30 drops twice a day, till he had taken 5fs. of the tincture, which increased his urine and relieved the breast. About the 10th of December he complained of a severe pain in the left side of the region of the stomach, which was removed by an epispastic, and the next day a violent pain of his head obliged me to bleed again, which together with a cathartic and epispastic inter scapulas removed the pain of the head. This blood was tough, fizy and cupping.

December 16. He was taken again with severe pain in his head attended with delirium: the next day when I saw him, he talked incoherently, did not know me, the pupils of his eyes were dilated, and he was averse to taking medicines or having any thing done for him; he, however, took a solution of tartarized antimony, puked several times, discharged a good deal of bile and found immediate relief, the pupils contracted and he soon became calm and rational. From this time he continued tolerably comfortable till about the 10th of January, 1806, when he had another severe turn of difficult breathing, could not lie down nor sleep in any position; he was bled largely, purged and afterwards puked, had blisters on his legs, took tinct. digit. cum vin. antimon. and again found relief. The symptoms for two or three weeks past, and more particularly

about this time seemed to indicate something of a change in the disease; the pain appeared to be more directly under the *middle* of the sternum than it had been, it came on very suddenly and went off as suddenly, which seemed, in some measure, to resemble angina pectoris, but the symptoms were not decisive of that disease.

He, however, about the middle of January, became more comfortable, and was able to ride in a sleigh with a good deal of satisfaction for two or three weeks; but about the 20th of February, his complaints increased upon him again, and he had issues made in the inside of his thighs, as recommended by Dr. M'Bride, for angina pectoris. When the issues began to discharge, he appeared to be benefited by them for some time, his breathing being easier and his sleep more calm.

About the middle of March, an anasarcaous swelling of his feet and legs began and increased, till about the first of April, when they were very much distended. The digitalis and other diuretics had been tried for sometime without effect. He now made use of an external application, recommended in Medical Repository, Hex. 2d. Vol. II. page 217, composed of soap, vinegar and spirits of wine, each equal parts, which soon reduced the swelling; the lower limbs and scrotum having been *greatly* distended and the inferior part of the abdomen considerably so.

He was now able to walk about again, which he had not done for sometime, but his right arm and hand began to swell soon after the swelling of the lower limbs had subsided, and was quickly removed by applying the above mentioned remedy. He continued with a variety of different feelings, and in the use of various medicines till about the 16th of April, when a great change took place in his pulse. There had been nothing

remarkable observed in his pulse for some time previous, but all at once it became so slow as to beat only 36 times in a minute, attended with a motion of the breast, at every pulsation, so strong as to move his clothes very perceptibly. He had not taken digitalis for sometime, but had taken calomel and opium in doses of two or three grains twice or three times a day for a considerable time, and had lately increased the use of the opium so as to take four or five grains at a dose, twice and sometimes three or four times a day. The pulse continued at 36 in a minute, generally very regular and pretty strong, till within three or four days of his death, when it increased to 40, but was not so strong or regular as when at 36.

April 28. I found him without any motion of pulse at the wrists, but he was able to sit up and walk about. The next day, April 29, a little before sun set, he got upon the close stool and was left alone for a few minutes; after this his mother went into the room, and found he had fallen off the stool and was *dead*, lying on his *face*, with his arms *under* his body, which seems to shew that he died without a struggle.

Thus ended this long and distressing case, the evening preceding an entire year from my first visit to him.

It ought to be mentioned that during his sickness, several physicians of eminence saw and prescribed various medicines for him, many of which have not been particularly mentioned.

It may also be proper to remark that medicines in their *usual* doses, had no sensible effect; but strong medicines in large doses generally afforded relief for a time.

## DISSECTION.

The next day, April 30, Dr. Sparhawk assisted me in opening the body. I made no minutes at the time, but from memory shall mention a few particulars. The skin, about the breast in particular, was considerably yellow, the fat was pretty thick over the sternum, and *very* yellow. On removing the sternum, we found the pericardium very thin and transparent, containing some water. The left lobe of the lungs was crowded, apparently by the increased bulk of the heart, close under the axilla, and left almost the whole of the heart bare, without any thing between it and the sternum, but the pericardium. The heart was greatly enlarged and on opening the ventricles, we found a large polypus in one of them. The right lobe of the lungs appeared to be sound, and in its natural place. There was some extravasated lymph in the chest. The liver appeared to be rather large and of a pretty firm texture. The gall-bladder was distended with a dark green bile, and contained a great number of calculi, or gall stones, some of them of a very irregular shape; one weighed xii grains.

The omentum was in a diseased state, of a darkish yellow colour, covering only a part of the intestines, and the fat mostly consumed.

## CASE II.

On Friday, the 15th of August, 1806, I was called to see a child of Mr. N. A. aged 20 months, and was informed by the parents, that on the preceding Tuesday, the child was observed to have a difficulty in making water, and that they soon discovered a redness and swelling about the pudenda, which had been increasing from that time.

On examination, I found the labia pudendi very much tumefied, attended with considerable inflammation both externally and internally; the clitoris was so much enlarged as to be partially erected. The internal parts had a singular appearance, being of a bright cherry red; some white matter appeared, and the whole of the pudenda was extremely tender and sore, as the child showed a strong aversion to having the parts touched; some eruptions, also, were to be seen about the groins and anus.

The child made water but once in 24 hours, and then with great pain, which commenced as much as an hour previous to the discharge, and continued for some time after, almost intolerable.

I at first supposed it a common inflammation, which by the discharge of the urine, produced irritation and distress. Prescribed a cooling purge, solution of gum Arabic and other soft drinks, to be given plentifully; applied emollient fomentations, and soft linen dipt in saturnine water, to the parts affected.

August 16. No abatement of the symptoms, the inflammation and swelling rather increased since yesterday; the same remedies were continued.

17. The inflammation still increasing; some excoriation of the parts having taken place, a wash made of a weak solution of sulphate of zinc was added to the former remedies.

18. Dr. F. was called in to visit the patient. He was at a loss how to denominate the complaint, having never seen a similar case in 50 years practice. The parts had now assumed quite a dark colour and seemed to threaten mortification. We observed a sore had made its appearance about this time behind one of the ears, and seemed to keep pace with those of the



pudenda, anus, &c. Bark was now prescribed, antiseptic fomentations, and the yeast poultice; some calomel was given internally, anodynes, and a wash formed by suspending calomel in a solution of gum Arabic; but the child continued to decline, a gangrene took place and she died in the night following the 24th, about the tenth day from my first seeing her, and the thirteenth from the first discovery by the parents.

### CASE III.

During my attention to the above patient, and only five days after my first visit to her, an older sister, aged three and a half years, was found to have the same difficulty and pain in making water.

By examination, on Thursday 21st of August, we discovered an inflammation and swelling of the labia pudendi; the internal parts, as in the former child, were of a cherry red and very sore, accompanied with considerable white matter.

We had some suspicions that there might be a *venereal* virus in the early part of the first child's complaint; now our suspicions were increased, and were very much confirmed by the opinion of another physician who was called in; however, on the most minute inquiry, we could not trace it to any probable source. We agreed, notwithstanding, to treat it as venereal; gave cooling purges, calomel in small doses, used the wash above mentioned made of calomel and solution of gum Arabic, and had it injected several times a day between the labia, kept the parts as clean as possible, applied emollient fomentations and Goulard's poultice to the parts; gave solution of gum Arabic plentifully, and other soft and diuretic drinks. On the 25th she made water with less difficulty, and twice in 24 hours;



the inflammation was at a stand, and continued stationary for several days; the same medicines were continued with the addition of the nitric acid, one tea-spoonful of which was put to a quart of water; she took half a pint daily of this mixture, sweetened with loaf sugar.

After a few days, perhaps ten, from the first attack, the inflammation began to abate very slowly; it was thought necessary, however, to continue the medicines for two or three weeks longer, when they were gradually withdrawn, except the acid and the external applications, which were continued till about the 20th of September, when the calomel wash was omitted. The parts still remaining very much excoriated and much redder than natural, with some degree of swelling; the wash of sulphate of zinc, and cloths dipt in saturnine water were continued.

October 11. This day I saw the child; she seems to be pretty healthy, eats and sleeps well, but there is still some redness and swelling about the pudenda, some foulness inter labia, and she walks rather stiff and awkward, but does not appear to have any complaint of the system. The acid and wash to be continued. No discovery has yet been made which can confirm our suspicions of a venereal virus, or account in any way for the symptoms above related. If the complaint was venereal, how could so young a child be infected? If it was not venereal, what was the disease? And how did it happen, that *two* such young children should have symptoms so similar and so near together as to time? The venereal disease does not happen very frequently in this place; but if it was venereal or any other contagious disease, might not the second child receive it by accidentally touching the fore behind the ear of the first, and then handling its own pudenda, as is very common for children to do?

December 10. The child is completely recovered.

*Account of an artificial Anus, formed in the Scrotum of a Hog; the natural Passage closing up. Communicated to the EDITOR.*

3d. mo. 4th, 1807.

IF the following fact should be thought worth inserting, it can be correctly authenticated.

A Boar being castrated, was found to have a rupture of the gut in the scrotum: no care being taken to sew up the part to secure the rupture, the gut continued to protrude; and finally, from some accident got broke; he was expected to die, but lingered for a long time, and, as is the practice of these wise animals, (physicians) when sick, never to eat any thing, for a long time; he at length crawled from his nursery a mere skeleton, when some attention was paid to him, and he throve and was fattened: it appeared when he was slaughtered for use, that the natural anus had entirely closed up, and the gut that had protruded at the scrotum, was converted into a rectum, and answered all the natural purposes.

The writer conceiving such a fact of importance, and that it ought not to be lost, communicates it; it will be evident that he knows nothing about medicine or technical terms: the owner of the hog, who killed it himself, is a man of strict veracity and communicated it to the writer, but would not like his name given to the public; neither would the writer. If Dr. Coxe thinks it worth inserting, in the Medical Museum, he is welcome to it.

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# MEDICAL MUSEUM.

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VOL. IV.....No. III.

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*History of a Case of Mania, successfully treated, in a series of Letters between DR. JOHN SPENCE, and DR. BENJAMIN RUSH.  
Communicated to the Editor by DR. SPENCE.*

No. I.

*Dumfries, July 16th, 1806.*

DEAR SIR,

**T**HE inclosed case, which is copied from my notes by one of my pupils, is transmitted for your consideration and advice.

Although I have frequently met with slight alienations of mind in puerperal women, this is the first case of Mania Puerperarum or Mania Lactea that ever came under my notice. I have therefore consulted such medical authors on the subject as are in my possession ; but their histories of the disease and their modes of treatment are far from being satisfactory. Nay, they often contradict each other,—one says “*rarus est morbus et ut plurimum insanabilis* ;”—another says, “*that the instances of its continuing six months are very rare, and there is scarcely a patient to be found that did not ultimately recover.*” In this number is the celebrated Dr. Denman.

VOL. IV.

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You will observe that the present case adds one to the many now on record, of the singular advantages of copious blood-letting in certain cases of difficult parturition;—a practice first recommended by you and afterwards ably enforced by Dr. Dewees.

Accept my thanks for your very ingenious inquiry into the Functions of the Spleen, Liver, Pancreas, and Thyroid gland, and believe me to be with great truth,

Dear Sir, your obliged  
And most obedient servant,

JOHN SPENCE.

DR. RUSH.



#### CASE.

MRS. J——B—— about 34 years of age, of a delicate habit, very irritable nerves, and subject occasionally to attacks of hysteria, was taken in labour with her first child on Thursday 5th June, 1806. On Saturday evening the 7th, I was desired to visit her. I found her very restless; her pulse full and slow; tongue white; skin hot; very thirsty; but her bowels sufficiently open. The Midwife informed me, that her pains had been pretty constant, but weak; that the waters had been discharged about twelve hours; that the head presented, but made no progress on account of the rigidity of the parts.

After having inquired fully into the circumstances of the case, I drew 24 ounces of blood from her arm by a large orifice, and although formerly disposed to faint when bled, there was not the smallest tendency to it on the present occasion, and I could not persuade her to sit or stand up. After this bleeding however, she fell asleep and on waking felt refreshed and less thirsty and feverish, but the labour pains were still considerable.

About 20 hours after bleeding her, having examined *per vaginam*, I found the head presenting closely surrounded by the *os uteri*, and neither of the ears could be felt. The external parts were much swollen and sore to the touch.

As she had now been in labour *four* days, had become extremely impatient and apprehensive, and her relations much alarmed, I again resolved to have recourse to copious blood-letting to induce syncope, and thereby relax the parts and facilitate delivery. I stated to her husband the object I had in view, and accordingly at 10 o'clock, P. M. I had her placed on the knee of an attendant, and again opened a vein in the arm. Whilst the blood was flowing freely, she was raised on her feet and supported by two women, and when thirty ounces were drawn, she began to complain of being faint; said she was ready to sink; looked very pale, and large drops of sweat covered her face;—her arm was now tied up, and she was laid upon the bed. A few minutes thereafter the labour pains came on with great force, and she was safely delivered, an hour after the bleeding.

The child was born dead; its head was unusually large; its limbs small and shrivelled: she thought it was alive at the commencement of labour, but if so it must from appearances have been in a languishing state. About five weeks before lying in, her little nephew when at play in her room, threw himself suddenly and unexpectedly into her lap, and struck his head with such violence against the region of the uterus, that she fainted away, and was attacked almost immediately with an alarming flooding, after which her breasts from being remarkably turgid and full of milk, became small and flaccid. It is probable, therefore, that the discharge of blood at that time, if it did not destroy the *foetus*, had materially affected its health; for there are perhaps no symptoms so strongly indicative of a child dying *in utero*, as the recession of the milk and flac-

idity of the mammæ. It is also probable, that the flooding proceeded either from a partial separation of the placenta, or from a rupture of some of the large vessels which run on its surface.

Wednesday, 11th June. For two days after delivery she continued easy and clear of fever, complaining only of great weakness and soreness when turned in bed: her weakness indeed was so great that she could not turn herself. The lochial discharge continues in moderate quantity. This morning (third day after delivery,) her breasts began to swell with milk; she became restless; talked at times incoherently; and when composed said she was about to lose her senses and would certainly run mad.

Directed an infusion of crem. tart. and fenna, and her breasts to be frequently rubbed with olive oil.

Thursday, 12th. Medicine operated several times; at ten o'clock last night she took 50 drops of laudanum, having slept none since Monday night. Soon after taking the opiate, she became very delirious, talked or sung incessantly, and when not watched would jump out of bed with great activity; suffered her breasts to be drawn by a servant girl, and rubbed with oil, but during the operation was very much agitated; called for some punch this morning, (by which she meant the fenna infusion,) two wine glass-fuls of which have opened her bowels.

Twelve o'clock at night. Has continued to rave constantly since sun-set, but does not refuse drink or food when offered her; has drank only water, water gruel and a few glasses of beer; has also ate some rice.

Friday, 13th, 2 o'clock in the morning. A little after midnight her feet and legs became quite cold, and her body, arms, and hands were covered with a clammy sweat. Pulse 100, and weak; no appearance of lochia; and the attendants think she has made no water for some time. Directed warm fomentations to the pubes and lower extremities, and after the fomentations, sinapisms to the feet.

Nine, A. M. Lower extremities became warm soon after they were fomented, and she complained much of the sinapisms. Apply a blister to the head and repeat the fomentations, should any coldness be perceptible in her feet. Directed 30 drops of the saturated tincture of digitalis in a saline draught, every three hours.

Saturday, 14th. Continued raving the whole of yesterday, without a moments intermission; took six doses of the drops, the last dose at twelve o'clock at night. Pulse fell to 74. She became more composed and slept about two hours; the first sleep she has had since Monday night; has had a stool and passed a great deal of water, both insensibly; on account of her restlessness, the blister on her head did not rise well.

At five o'clock this morning, the maniacal symptoms returned with greater violence than ever; four women with great difficulty held her in bed. Pulse 100; but its frequency is greatly affected by her exertions.

Without pursuing the daily reports farther, suffice it to say that about this time she began to take camphor. The doses were gradually enlarged till she took 120 grains in the course of six hours. Though this medicine was exhibited at two different periods, at each period it seemed to induce a feverish state, and aggravate the confusion and tumult of the mind. She would struggle with her attendants; cry out her stomach

was on fire, and call for water to drink. After laying the camphor aside, I again had recourse to the tinct. digital. in large doses; it reduced the pulse to 54, but had no effect whatever on her mental derangement. *Affæoetida* and other antispasmodics were also tried and given to great extent, but with no better effects. Laudanum in doses of 50 drops increased the violence of all the symptoms, but from two to three hundred drops always composed her, and induced sleep for several hours; but on waking from this sleep, her strength appeared to be renovated, and she would sing and talk louder than ever. Blisters to her head and ankles, cold applications to the head, and the shower bath, have also been successively had recourse to, but she was generally so ungovernable that these applications could not be said to have had a fair trial. Whilst these various remedies were employed, her bowels were kept open by saline cathartics.

On the 7th July, the menstrual discharge came on, and was considerable nearly three days; from this evacuation great relief was expected, but unfortunately no change whatever ensued. From the 20th June to the present time, (15th July,) she has been pretty clear of fever, has had a good appetite, and when relieved from confinement would dance and jump about the room, laughing and singing all the while, as if greatly delighted.

A few days ago when she was seized with a violent fit of raving, I bled her until she had nearly fainted. Soon after the operation, however, she began to sing and talk, but her relations thought she was upon the whole much calmer after the bleeding. She has used only a vegetable diet; her drinks have been simple water, water gruel, or weak tea. Her chamber has been kept as dark, quiet, and as cool as the season would permit; no medicines except laxatives have been ad-

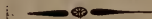


ministered for the last fortnight, and during that period she has slept a few hours almost every night.

Since she became deranged she has not had a single lucid interval; for although she knew and could name all her acquaintance who entered her room, yet to none of them could she give a distinct answer when a question was asked. It is true indeed that during the early stages of the mania, and during the secretion of the milk which continued but a few days, she talked without ceasing about the child—would seize the pillow, press it to her bosom, and sing lullabies. There seems little doubt that the loss of her child contributed to bring on her present unfortunate situation. She has been married eight years, but was never pregnant before; and both she and her husband looked forward with delight to an increase of their family.

JOHN SPENCE.

*Dumfries, 15th July, 1806.*



No. II.

*Philadelphia, July 19th, 1806.*

DEAR SIR,

**D**ID all our young physicians record the history of the diseases they meet with, with the effects of remedies upon them, after the manner you have done in your account of Mrs. B's case, they would find their journals and commonplace books more useful to them in the evening of life, than any of the books that belong to their libraries.

Parturition is frequently followed by madness. I have seen many instances of it. At present I am attending a lady from

Carolina in whom that disease was induced by several difficult labours followed by the birth of dead children.

My remedies for madness in its first stage, or rather in *all* its states of high excitement, are bleeding, purging, and extremely low diet. In the use of the lancet, I am more influenced by the high temper, and extravagant conduct of the patient—by the fierceness of the eyes—by the absence of sleep, and by cold feet with the above symptoms, than by the state of the pulse. In all diseases of the brain Mr. Hunter has well observed the pulse now and then refuses to impart any knowledge of their force, seat, or danger. Where the strength of the disease is concentrated in the brain, and other parts of the system are languid, I have substituted cups and leeches to venesection. Perhaps it is not too late yet to use one of those modes of bleeding in Mrs. B's case. If it be, purges of calomel and jalap should be given. The liver, spleen and bowels all require, from the congestions induced in them before and after madness, the occasional use of this class of medicines.

Blisters to the *ankles* I have found more useful than to the head and neck before the excitement of the brain is in part reduced. I learned this practice from Dr. Willis soon after he cured the king of Great Britain.

If all the above remedies fail, recourse must be had to a salivation. There is no general disease in which its effects are more uniformly beneficial. I have known *order* to be established in the operations of the mind in a few hours after the patient complained of a fore mouth. But a much longer time is generally necessary for it to cure.

The cold bath should follow the salivation, if any part of her disease should remain unsubdued, also cordial diet and medicines.

Great attention should be paid to the state of her mind. Every thing associated with the cause of her disease, should be removed, and objects calculated to revive pleasant and healthy associations of ideas should be sought for in company—in the haunts of her youth—or in any other place or situation in which she has been happy.—When her temper, or wrong ideas are highly extravagant her mind should be soothed or *diverted*, as if by accident, to other subjects.—When her alienations of mind become more feeble, they may be *opposed* by reason—by amusements—by conversation—and even by ridicule,—each of which I have known suddenly or gradually to restore order to the actions of the mind. Gentle exercise, when practicable, must not be neglected.

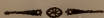
I was much pleased to read your account of the good effects of copious bleeding in facilitating Mrs. B's. delivery.

It will give me pleasure to hear of the issue of Mrs. B's. case. I more than hope;—from the present symptoms, and prognosis of her disease—I expect her recovery. Madness, succeeding hysteria, and attended with good humour, is much less difficult to cure than when it originates in hypochondriasis, and is attended with silence, and depression.

From, dear Sir, yours,  
very respectfully,

BENJAMIN RUSH.

DR. SPENCE.



### No III.

**I**N the following table the first column shews the month—the second the day of the month—the third, the number of grains of calomel given each day, and the fourth, the number of stools in 24 hours.

*Mr. B——'s Journal of Mrs. B——'s Case.*

<i>Month.</i>	<i>Day.</i>	<i>Calom.</i>	<i>Stools.</i>	<i>Remarks.</i>
July.	24	6	4	} Sings and talks, laughs and dances a great deal. This day began to spit.
	25	6	4	
	26	6	3	
	27	6	6	
	28	3	5	} Mouth very sore, eats nothing; tranquil, and very silent.
	29	1	12	
	30	1	6	
	31	1	0	
August.	1	0	0	Dozed half the day, when awake peevish.
	2	0	0	Salivation very profuse
	3	0	1	Drank a little milk and water.
	4	0	0	Salivation still very copious, can scarcely swallow.
	5	0	0	Takes gentle purges.
	6	0	0	Mouth mending.
	7	0	2	Begins to talk.
	8	0	0	Did not sleep well. [the day.
	9	0	1	Slept well, but talked much during
	10	0	1	Did not sleep till day break.
	11	2	1	Being violent, was bled to 15 oz.
	12	2	2	More moderate; slept well.
	13	4	3	Still moderate; but did not sleep well.
	14	2	5	Slept tolerably.
	15	4	3	Rather violent.
	16	6	6	The same.
	17	6	0	Appearing sick at the stomach, took an emetic which operated well.
	18	3	0	Did not sleep till 2 A. M.
	19	5	8	Slept none.
	20	6	5	Slept well.
	21	6	7	Slept well.
	22	6	2	Did not sleep till day.
	23	5	1	The same.
	24	12	1	} Sleeps but little. Began to spit.
	25	15	0	
	26	6	2	
	27	6	1	
	28	6	2	} Spits a little.
	29	8	2	
	30	5	2	
	31	0	3	
Sept.	1	0	3	} Spits a little.
	2	0	2	

Sept.

4	5	2
5	0	2
6	5	1
7	0	1
8	1	1
9	5	1
10	0	1
11	5	2
12	5	2
13	6	0
14	12	2
15	10	4
16	9	3
17	10	4
18	10	5
19	10	3
20	10	2
21	10	0
22	11	1
23	11	1
24	11	3
25	11	2
26	11	3
27	11	2
28	11	2
29	11	2
30	11	2
Oct. 1	12	2
2	12	1
3	22	1
4	22	1
5	12	2
6	12	1
7	12	2
8	12	1
9	12	2
10	12	2
11		
17		

Salivation still inconsiderable.

Moderate, walked out.

Walked out, but very wild.

Wild.

The same.

The same.

Salivation still trifling, breath offensive.

Began the shower bath twice a-day.

Shower bath continued every day, but she submits to it with great reluctance.

Omitted calomel.  
Left off the bath.

Salivation increased remarkably after leaving off the mercury, mouth and throat became very sore, breath offensive, bowels since the same period have been kept open by stewed fruits, or molasses and water.

Has rode out several times in a carriage; though often silent, never gloomy or melancholy.

In the foregoing table the number of grains of calomel given each day is stated; but this quantity was always exhibited in divided doses. The doses were carefully weighed by her husband and given to her by his own hand.

The cask containing the shower bath was elevated nine feet, and held ten gallons of water.



No. IV.

*Dumfries, October 30, 1806.*

DEAR SIR,

THE above is a journal of our patients' case by her husband, which I desired him to keep, as my engagements during the sickly season prevented me from attending to her minutely. In a medical point of view this journal is imperfect, but I deem it valuable and therefore, although rather prolix, I think it proper to send you an exact copy. You will see by it that I commenced a mercurial course as soon as your instructions came to hand. You will observe also that the calomel soon after it was administered operated powerfully as a cathartic thereby answering one great object you had in view,

that is, in removing those "congestions in the liver, spleen and bowels, so often induced in them before and after madness."

She has been salivated *twice*, and in each salivation the effects of the mercury on her system were so different and so strongly contrasted as to merit notice.

In the first instance a small quantity of calomel (29 grains) soon excited considerable spitting, and at the same time purged severely. The salivation soon became as profuse, the gums and fauces as much swelled as I ever saw from mercury. Indeed from the 31st August to the 7th September, deglutition was almost entirely obstructed. During this severe salivation she was so costive as to require laxative medicines: When the salivation began to abate her mouth mended rapidly.

On the contrary, when the calomel was again administered, more than 100 grains were given before she began to spit, nor were her bowels much affected by it. When the largest quantities were given, the number of stools might be said to be in an inverse ratio to the number of grains exhibited; nor was salivation increased by these immense doses. Few or no stools, no sweats, and little salivation under such circumstances astonished me, inasmuch that I doubted whether such large portions of calomel were really given; but I was soon convinced the statements were correct. On more minute inquiry I found that her attendants were obliged to change her dress and bedding very often. She passed great quantities of water in bed, and to all appearance involuntarily. The mercury, therefore, must have stimulated the kidneys, and run off by the urinary organs. Another circumstance during the second mercurial course is remarkable. About three days after the calomel was omitted a copious salivation with swelled gums and jaws came on, and continues to this day (30th October,) though now abating. Her left jaw is still much swelled, and

her breath offensive. Her urinary evacuations have diminished, and her bowels are kept moderately open by molasses and water, or stewed fruit. There has been no return of the catamenia.

With respect to the state of her mind, when the salivation became in the first instance profuse, I had great hopes of success; for instead of singing, laughing and talking almost incessantly, she was tranquil and silent; but when her mouth began to mend she became as wild and irascible as before.

Since the last salivation commenced she has slept well, has been more composed, and is not now violent more than once or twice a day, and then she appears to be in a passion on account of something she imagines unjustifiably withheld from her; but when she obtains what she calls for she rejects it with disdain.

When the shower bath was first used she exclaimed, "This will save my life;" but soon afterwards she was so violently opposed to it, that a desire was expressed it should be left off until I heard from you again. While using the bath her hands, arms, and feet, from being generally cold, became warm, the warmth approaching to a feverish heat.

Every liquid she gets hold of (even her saliva) she rubs on her head, as if her disease lay there. Her eyes have a muddy appearance, like those of a person who has been fatigued, and watching for many nights. She sees and hears with great acuteness.

Although I have said that she has lately been more composed, yet she has not had as yet a distinct lucid interval; and her relations despair of her recovery. You will observe my



reliance hitherto has been almost entirely upon the mercurial salivation; being anxious to see its wonderful revolutionary powers fully tried before recourse was had to any other remedies.

Her diet has consisted principally of gruel, thin soups, stewed fruits and preserves. She is very fond of molasses and water, and, as has been already observed, when she drinks it freely, it operates as a cathartic. But when I call to my recollection all the food she has taken since the mania commenced, I really think it would scarcely suffice to support a weaned infant the same number of months; and, though now emaciated to a degree that astonishes those who were formerly acquainted with her, she still retains great muscular strength. She sings louder and more melodiously than she ever did, and remembers every word of every song she could sing when in health. I have several times had her brought to her harpsichord; but she could never play any air, except *God save the king*, the first perhaps she ever learnt. When she attempted other pieces of music and failed, she would either get into a fit of raving, or would rattle away on the keys, laugh exceedingly at the motion of the jacks, and indulge in all the effusions of clamorous mirth.

Should the foregoing statement, which I fear in some things is needlessly minute, enable you to form an opinion of Mrs. B's present situation, I beg you will advise what is further to be done. And should any symptoms be omitted, which might lead to a clearer conception of her disorder, I hope you will put such queries as you may think necessary, and I shall answer them as fully as I can.

I remain, dear Sir, your most obedient servant,

DR. RUSH.

JOHN SPENCE.

## No. V.

*Philadelphia, November 8th, 1806.*

DEAR SIR,

THE salivation has probably effected all that can be expected from it in our patient's case.

Long after *morbid action* is reduced in the arterial system which pervades the trunk and limbs, it continues, though often in a feeble degree, in the brain. This I suppose from your history of Mrs. B's. symptoms to take place in her brain. The remedies for it should now be cold applications to her head and warm water to the feet at the same time, thus to equalize, if possible, arterial and nervous excitement. The cold applications may be ice pounded and confined in a bladder, cold water, or Dr. Cullen's clay cap. If this remedy be objected to, local depletion by cups, or blisters to the head may be useful. If her pulse be active in the temporal arteries, or if she continue to exhibit signs of great excitement in her brain by the ferocity of her face, or the violence of her temper, it will perhaps be best to use the depleting remedies, before the cold and warm applications.

Cordial medicines and diet should be given if her pulse do not forbid it. Opium, above all things is the *medicina mentis* in cases of languid excitement in madness.

New actions should be excited in her mind by conversation, books or amusements. A cure of madness was once performed upon a lady in England, by confining her for hours to a card table.

Adieu, from dear Sir, your friend and brother in the republic of medicine,

BENJAMIN RUSH.

No. V.

*Dumfries, February 15th, 1807.*

DEAR SIR,

IT will be no less gratifying to you to hear, than it is to me to communicate the pleasing tidings of Mrs. B's. recovery. The prognosis you had formed of her disease in its early stage has been fully verified.

Previous to the receipt of your last letter attempts had been made to use the clay cap; but such was her violence that these attempts were made in vain; and there unfortunately existed among her relations an almost insuperable aversion to any thing like a strait waistcoat. Although I always acquiesced in the propriety of indulging her in every innocent wish and inclination, yet I conceived this indulgence to be carried beyond proper bounds whenever it interfered with the use of efficient remedies. I pointed out the impossibility of complying with your directions unless some restraint was used, and at last succeeded in having a tight gown constructed for this purpose. This gown was put on the 3d day of December, when the hair was cut off as short as possible, and snow applied to her head in a large bladder, and ordered to be renewed frequently. Her lower extremities were also directed to be bathed in warm water for an hour at a time every evening, and afterwards well rubbed. This operation of bathing pleased her much. She did not at first complain of any uneasiness from the snow, the application of which to the bare head of any one but a maniac would I think have been insupportable. After the snow had been carefully applied for about sixty hours, she asked why her dress was so tight; and on being told the cause, she promised to let any application remain undisturbed, provided the tight gown was taken off; and for some days she kept her promise. This was the first time we discovered the dawn of returning reason; she now stated the

great disorder in her brain, and how much she was distressed by loud acute sounds. During the whole period of her indisposition, indeed, when any person addressed her in a loud voice she would clap her hands on her ears and call out "echo, echo." She now too complained of the snow, and when the clay cap was substituted for it, she appeared highly pleased. This cap was frequently wetted with cold vinegar and water; but in a few days I found it necessary, in consequence of the recurrence of unusual fits of outrage, to direct bags of snow or ice to be applied over the clay, and again with good effect. During this course lucid intervals became longer and longer every day; and about the commencement of the present year she was so rational, that the cold applications to her head, and warm bath to her feet, were discontinued.

On the restoration of her reason she complained of great weakness, particularly in her joints, and was remarkably grave, so much so that her relations were apprehensive it would terminate in melancholy. I advised exercise in the open air, and such cheerful company as she *seemed pleased with*. By the bye this is a circumstance I found it necessary to be particularly attentive to; for when she went into the company of strangers, they gazed at her with a degree of eager curiosity, that both astonished and distressed her, and was perhaps in some measure the cause of that gloom which succeeded her indisposition. The only medicine I directed was the rust of steel in pretty large doses. Since that time she has had a return of catamenia, though rather in small quantity; she has enjoyed also sound and refreshing sleep, and has recovered much flesh. At present her spirits and her appetite are good, and upon the whole I consider her recovery complete.

As to what passed during her illness, she supposed she had been sick only two or three weeks; and I discovered that she had no knowledge of the disease under which she had laboured,

had no recollection of having been salivated, nor did she remember any occurrence of consequence. She remembered only the shower bath and how nearly she had fainted the last time she was bled. She also had a distinct recollection of every event preceding the maniacal attack, and the first part of her furniture she examined after her recovery was the drawer containing the clothes she had prepared for her infant.

You see I continue fond of detail ; but to me this case is new and has been very instructive. I am pleased therefore that I have recorded it so minutely. From the vague and obscure manner in which puerperal mania is described and treated by the writers I have had an opportunity of consulting, I felt at a loss what to do ; and this difficulty was not a little increased by my total want of success with medicines celebrated for their sedative powers, or for moderating inordinate nervous excitement ; and it may be supposed that I administered these medicines with a degree of boldness not altogether consistent with prudence. In this state of doubt I expressed a wish to have your advice, and it was well I did so ; for your letters pointed out clearly the proper steps to be taken. These letters, though short, are perspicuous, and inculcate a mode of practice at once simple, rational and energetic, and discover an intimate knowledge of one of the most distressing and humiliating diseases that afflict human nature.

Mrs. B. desires me to express to you her sincere gratitude. Mr. B. also joins in compliments to you, and begs your acceptance of the enclosed ——— as a small compensation for your prompt and kind attention to Mrs. B.'s case.

I am, dear Sir, with unfeigned regard and esteem,  
Your obedient humble servant,

JOHN SPENCE.

DR. RUSH.

## No. VI.

*Extract of a Letter from DR. RUSH in Reply to the above.*

“ I HAVE received your very satisfactory letter. I took the liberty of reading the history of Mrs. B.’s cure to my class. It served to confirm the practice I had recommended a few days before in my lectures, and which I had illustrated by six successful cases out of ten of a similar nature in our hospital during the winter. One of them was a man of 68 years of age, and of a family subject to madness. The disease in this patient was uncommonly violent, but it yielded to *seventeen* bleedings, (ten or twelve ounces at a time) to purges every second or third day and to low diet, in *six* weeks. He is now with his family in New Jersey in good health.”

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*Remarks on DR. PEACHEY HARRISON’S Reply; by WILLIAM P. DEWEES, M. D.*

*Philadelphia, August 1st, 1807.*

DEAR SIR,

IN looking over the last number of your Museum, I observe a reply from Dr. Harrison on the subject of impregnation. I would willingly for the present wave the farther consideration of this subject, did I not feel I owe Dr. H. some apology for the manner in which his “Observations” &c. were treated.

To you, sir, my reasons for the mode I pursued are well known, but it is not so with Dr. H.; I therefore will briefly state them, and trust his candour will give full credence to what I advance. You will readily recollect I was

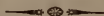
under the persuasion, that "Peachey Harrison" was an assumed name, from being informed, and as I believed, on good authority, that the paper entitled, "Observations," &c. was written by a medical gentleman in this city, who chose this signature with a view to disguise from what quarter the piece came; the motive would have been evident, had this proved true. I called on you with a view to make some discovery on this subject by inspecting the post mark, place of abode, &c.; but sir, upon reference to the envelope we could find nothing satisfactory, nor was the paper itself dated, as you have since borne testimony in a note to it.\* This was conceived a strong corroboration of what I had before heard, and I acted agreeably to feelings thus excited. I trust this statement will satisfy Dr. H. that it was not sheer wantonness that urged me to the stile of reply I adopted.

Feeling this explanation necessary through the medium of your Museum, I have added to it some remarks on Dr. H.'s answer, and if sir, they in every respect square with the objects of your work, I hope you will also give them publicity.

I am, Sir, your's sincerely,

WILLIAM P. DEWEES.

DR. JOHN REDMAN COXE.



DR. H. in his reply observes, p. 98, that "it cannot be regarded as presumption in me to believe that the Doctor has embraced an hypothesis," &c.; on the words "embraced an hypothesis," he remarks in a note "I say embraced, because I am able to shew he is not the inventor of it." The inventor of what! I would beg leave to ask? Have I claimed originality for the doctrine of the possibility of superfœtation? certainly I have not, as Dr. H. may very readily satisfy himself by turning to the essay; he will find I say it "is not a

\* The Editor hopes the circumstances here detailed, will sufficiently evince the attention which is necessary to authenticate by date, and residence, &c. such communications as are transmitted to him for publication.

new idea," and that the express object of the essay "is to revive it." Nor do I any where claim the doctrine of absorption; as every one knows, who is in the least conversant with the subject of impregnation, that this honour is due to Harvey. It is true, I have offered a modification of this opinion, by supposing this operation (absorption) may be affected by a set of vessels whose sole duty it is, to convey the ejected semen to the ovaria; and this, of however little consequence it may be, or however absurd it may appear, I believe to have originated with myself; and I also believe, that no one before me, has attempted to deduce the possibility of superfœtation, from the premises I have adopted. From this, it would appear, that I claim but the humble merit, of arranging in a new order, facts, and opinions, known to many before me; and this I shall continue to believe, until I am shewn, that I am not entitled to priority in this arrangement.

Dr. H. is certainly wrong in supposing, my object was to call in question the classical purity of the expression, of "extorting from nature her oracles" in my remarks on it; it was done with a view to set him against himself, when in one place he flatters himself he has done this (extorted nature's oracles,) not altogether without success; and in another, launches into an ocean of conjecture, which eventually I think overwhelms him; as the oracles of nature should be pure and unfulfilled by conjecture. Am I not then correct, when he substitutes hypothesis for the "oracles of nature" in saying, they, in my opinion, are without foundation or plausibility? In the same paragraph, p. 100, Dr. H. observes "may I be permitted to inquire, upon what foundation his (Dr. D's) hypothesis rests? —upon facts? I declare, that after reading his essay upon su-

\* Dr. H. need not have had recourse to the Mantuan bard for the employment of the term, as Spallanzani, much nearer our time, makes use of the same words and for the same object. He says, p. 244. *differ. II. Par. clxxi.* "In this branch of physics, we must not generalize our ideas, but are under the necessity of consulting the oracles of nature, and receiving her answers with respect and attention."



perfoetation repeatedly, and with the utmost attention of which I am capable, I am unable to find any from which his hypothesis could directly be inferred : and he confessedly admits, that he takes those facts for granted upon which he builds his hypothesis. Oh ! but he first proves, that superfœtation cannot take place according to another hypothesis, and therefore it must take place according to the hypothesis he has suggested ? What kind of philosophy is this I beseech the Doctor ? Has he learned this mode of reasoning from Lord Bacon, or from Sir Isaac Newton ?"—There is in this quotation an ambiguity and want of consistency that is well worthy of attention. I cannot understand the Doctor, when he says I have confessedly admitted I have taken my facts for granted. Have I not given authorities for the major part of them ? they are therefore not assumed ; and if I understand the import of the word assumed in this instance, it means, I have declared, I have taken for granted, circumstances that may not be true ; now no man in his senses, be his sincerity what it may, would declare he did not believe in his data, when he was about to draw an inference important to his doctrine. But I will go farther and here declare, I do believe implicitly in the facts quoted, and consequently do not beg them. Is it not "illogical" to conclude that facts must be "assumed," because Dr. H. cannot find any, from which my hypothesis can be directly inferred ? This does no more than call in question the force of the facts, but surely does not destroy their truth. Besides, Dr. H. admits a little farther, that I prove "superfœtation cannot take place according "to another hypothesis," yet affects to call in question the facts on which this proof rests ; how does this harmonize ?

I shall now, for a moment advert to a part of the above quotation, on another score. Dr. H. says, in a manner sufficiently unqualified, that I have concluded, because superfœtation cannot take place according to another hypothesis, that it must take place according to the one I have suggested. I do declare

I have said nothing like it, nor is it in his power, with all his logical talents to aid him, to shew, that such a conclusion will follow my premises. Were it so, the Doctor might well exclaim, "What kind of philosophy is this." I trust I have, to every unprejudiced mind, shewn how it cannot take place; and then have humbly attempted to shew how it were in my opinion possible; if this be a crime I plead guilty; but I conceive every man has a right to call in question, doctrines however long received, or however sanctioned; but in doing this, it is also incumbent, to offer one in the place of that he has endeavoured to repudiate. This I have done, but not in the dogmatical style Dr. H imputes to me. Dr. H p. 101, thinks he has ascertained from "experiments, repeated frequently, patiently, and perseveringly, and instituted with a view to ascertain the point, that venereal sensibility has its principal seat in the os tincæ;" but acknowledges he is unable to support the fact by the direct testimony of any authorities to to which he *has access*. Does he know of any authority either ancient or modern for the assertion? I must confess I do not. He then asks, was it in the power of "Harvey to substantiate the fact of the circulation of the blood, by the testimony of other writers?" I say yes, and upon those who had gone before him chiefly; it was upon their discoveries, and facts, that he first deduced, and then proved the circulation. Had no one preceded Harvey, had he not had multiplied experiments, and facts to to have rested on, he would not have immortalized himself by his discovery. Did he discover that the heart had two auricles and two ventricles, which contracted and forced the blood from it? Did he discover there were two sets of vessels belonging to the sanguineous circulation? The same question may be asked of almost all the anatomical facts connected with this discovery. Harvey has then the merit of employing the knowledge of other anatomists, so as to produce the most important discovery in the history of medicine. And had not Jenner had public and living records, to prove that the

natural vaccine disease when received into the human constitution prevented the small-pox ; we should not now have been benefited by his discovery that this obtains equally, when artificially introduced.

Dr. H. p. 102 appears to have some difficulty in understanding what he terms, a “perplexed chain of propositions;” this I conceive must be his own fault, since I will venture to say, they stand in no such predicament with any one else ; they appear to me plain and conclusive, and this I assign as the reason why Dr. H. found them perplexed.\* But I would beg leave to ask by what rule of grammar or “logic” will Dr. H. make “very many” mean most ? I have not ventured to rely on my own judgment with respect to what I conceived to be the obvious meaning, but have taken some pains to inquire of several of my literary friends, on whose opinions, as far as regarded this question especially, I could safely depend, without being able to find any authority for Dr. H.’s construction. But, least this should again be cavilled at, I now explicitly declare, that my meaning was, that a considerable number, was intended to be understood.

In p. 103 Dr. H. leaves to logicians to decide, whether the inference drawn from “the perplexed chain of propositions” be properly made from the premises ; “and whether the reasoning, if it conclude against any thing, does not more logically conclude against the necessity of pleasure in the prolific intercourse, than against the feat of the venereal sensibility being in the *os tincæ*. If there are indeed women, who, when they are in a situation to be impregnated, feel no pleasure in the sexual intercourse, and are yet prolific, I grant this proves that pleasure is not an indispensable requisite in fecundation.”

Let us now examine Dr. H.’s theory of impregnation, and see how it will square with the above concession. Dr. H. p.

\* Or rather perplexing.

423. Vol. II. No. viii. gives "the following combinations of causes and effects, to convey a *clear conception* of the manner," in which he supposes impregnation to be effected.

"Irritation applied by the glans penis to the os tincæ, well prepared to receive this irritation by its projection into the vagina, and by its extreme sensibility, is the immediate cause of the venereal orgasm, or that unknown desire which the female sometimes feels, in coition, for the feminal stimulus; and secondly, that an *absorption* of the fecundating fluid, when applied to the os tincæ during the orgasm, is the proximate effect thereof; and in the third place, that the transmission of the semen to the ovaria, *by the proper action of the uterine system*, is as naturally the consequence of absorption, as deglutition is of agreeable aliment taken into the pharynx. And lastly, that impregnation is the final cause of the sexual intercourse, and of the *pleasures* with which it is accompanied."

"It clearly follows from what has been stated above, that impregnation *will never take place*, unless the venereal orgasm has been excited; and it is equally plain, that it will not happen, unless the semen is brought in contact with the os tincæ during the venereal orgasm; and it follows with *equal clearness*, that an apposition of the urethra to the os tincæ, at the same time will be important, if not indispensable; not indeed for the purpose of the injection of the semen into the uterus, but that the feminal stimulus be applied to the os tincæ, at the moment when it is prepared to absorb and carry it to the ovaria."

The opinion of Dr. H. is then "so far as I am able to understand, so perplexed a chain of propositions" first, that contact between the "velvet like head of the penis," and the os tincæ, is essentially necessary, as appears from the expressions, "irritation applied by the glans penis to the os tincæ"; by its

being "well prepared to receive it;" by its (the os tincæ) being "actually the seat of pleasure in females;" and from its "in the sexual intercourse" receiving "irritation from the soft and velvet like head of the penis," &c. Now we have in our former reply, said enough from the most respectable authority, to convince any unprejudiced man, that many instances of impregnation have occurred, where it was physically impossible for this contact to take place. But lest it should be objected, that these obstructions may have arisen after impregnation had taken place, I will give an extract from Baudelocque's Midwifery, on the authority of Barbout, that I conceive perfectly in point. "Transverse partitions have often been found in the vagina; and it has been known to open into the rectum, in women who wanted the external parts of generation, without this vicious confirmation having rendered them absolutely barren." Barbout, tom. I. p. 59.

This, with the examples we before cited, with the generality of men, would be sufficient to prove that instances of impregnation have occurred, where "irritation by the glans penis" could not be immediately communicated to the os tincæ; but to do away all pretence to this essential, I will relate the following history, the truth of which may be relied on, as it was communicated to me by the lady herself, who certainly had no theory to support.

Mrs. G. aged 33, was seized on the 10th April, 1807 with a violent vomiting in consequence of a portion of intestine becoming strangulated in a hernia, which she had laboured under many years; I was called to visit her, and in a few days she perfectly recovered; during my attendance on her she mentioned, she had ever since her first labour been troubled with a falling down of the womb, for which she was obliged always to wear a pessary. I begged her to shew me one of her pessaries; it was made of cork coated with wax; it was about

four inches long, two broad, and had a hole in the middle of the third of an inch in diameter. She had worn one ever since her first lying in, with the exception of a short period after each delivery, and after the sixth month of each pregnancy. She was the mother of six children. As this case was so directly in favour of the idea, that contact between the penis and os tincæ was not necessary to impregnation, I was particular in my inquiries respecting the most essential circumstances. I asked, if she always wore the pessary as she had asserted, (with the exceptions just stated) how could she possibly have become pregnant? she said she did not pretend to know how this happened, but such was the fact. Am I then to understand, without any equivocation, that you believe you ever have become pregnant while you wore the pessary? She assured me, she was certain this had always taken place while it was within her, with the exception of the first child. I demanded, whether this was not an obstruction to conjugal union; she said, nothing like as great as without it, for her womb was always so low, as to prevent entirely this intercourse.

Secondly, Dr. H. asserts, that from "its extreme sensibility, (the os tincæ) it is (in conjunction with irritation) the immediate cause of the venereal orgasm;" now as we have, we think sufficiently proved above, that impregnation has taken place where this irritation could not be applied to the extremely sensible os tincæ, no venereal orgasm could take place, as its proximate causes were wanting; consequently the venereal orgasm, or that "unknown desire which the female sometimes feels in coition, for the feminal stimulus," cannot be essential to impregnation, if, as Dr. H. supposes, the glans penis and os tincæ must be in contact for this effect to be produced.

I think, I do not force Dr. H.'s meaning, when I assert, he must consider pleasure, or pain, essential to the venereal orgasm,

and consequently to impregnation ; since, agreeably to his own declaration, "impregnation will never take place unless the venereal orgasm has been excited." He admits the glans penis to be an irritating body ; the os tincæ to be an extremely sensible body ; now, what is the necessary consequence of the application of a body capable of irritating, to another body extremely sensible, must it not be either pleasure or pain ? It is true Dr. H. has urged p. 422 "that the venereal orgasm consists in a certain excitement of the uterine system, and is accompanied with exquisite sensations, similar to those which take place in the male ;" but as I cannot perceive the difference as respects the main or "final cause of sexual intercourse," whether the pleasure attending the act, be the cause or effect, or neither, since it must accompany, and be present at the time the venereal orgasm exists ; and I care not to which of these heads he refers it, since he agrees, that impregnation cannot take place "without venereal orgasm," and that the "venereal orgasm is accompanied with exquisite sensations." Pleasure then, agreeably to Dr. H.'s hypothesis is a necessary link in the chain of effects, arising from the irritating glans penis, when applied to the extremely sensible os tincæ.

Thirdly, that the semen must be absorbed during the venereal orgasm. It might not be amiss to ask Dr. H. how he supposes this absorption to be effected ; for he certainly has not explained it, by saying it does happen. His ambiguity and want of logical precision on this subject are very notable ; for he conveys nothing certain to my mind when he says "that the transmission of the semen to the ovaria, by *the proper action of the uterine system*, is as naturally the consequence of absorption, as deglutition is of agreeable aliment taken into the pharynx." For, before we can admit this opinion, several things must be proved : first, that the venereal orgasm is produced in the manner urged by Dr. H. : secondly, that this, is essential to impregnation ; thirdly, if it does take place as Dr. H. supposes,

he must shew that this part, when so circumstanced has the power of absorption ; and fourthly, that when it has absorbed, he must demonstrate it conveys the semen along in a manner analogous to swallowing, or that this *proper action* of the uterine system takes place.

Dr. H. p. 102, reduces my arguments (or agreeably to his own phraseology) my “perplexed chain of propositions” against the seat of venereal pleasure being in the os tincæ to a dilemma, which he thus retorts, “now, unless that those women who enjoy pleasure in the venereal congress, and those who do not, have an organic difference in the part in which the venereal sensibility resides, we must conclude that *this part* is not the seat of venereal sensibility.” This dilemma of the Doctor’s might easily be again retorted, but as I have not pretended to locate venereal sensibility or enjoyment, in any thing I have advanced, and as I do not believe in its instrumentality in conception, the onus probandi lies with him.

Dr. H. p. 104, endeavours to draw a parallel between the sense of feeling possessed by the hog in his snout, and the elephant in his proboscis, and the sensibility of the os tincæ ; since he evidently confounds the sense of feeling, which these parts may have, even in an eminent degree, with sensibility strictly so called, I shall only observe, in general, the analogy between the sense of touch which distinguishes between bodies, and that state of a part termed sensibility, is too remote to serve as a standard of comparison, and too distinct ever to be confounded. Dr. H. a little farther on, says, “to these observations, (those respecting the hog’s snout and the elephant’s proboscis) it is hardly worth while to add, that its texture (the os tincæ’s) is certainly less firm than that of our teeth, which are as sensible to the variations of heat, perhaps, as any other part of the body.”



It will be readily admitted, that the teeth are very sensible to cold, but not to heat,\* unless they are diseased. But I will not allow, Dr. H. has any right to the comparison, since the bony substances of the teeth are mechanically acted upon by the contraction of the enamel which furrounds them. In consequence of the contraction which the enamel suffers from a reduction of temperature, the nerve of the tooth is impinged upon, and no one will deny but a living nerve possesses sensibility.†

I am sorry to be under the necessity of accusing Dr. H. of mis-quoting me in the next paragraph; I am rather disposed to believe, from his general appearance of candour, that it must have proceeded rather from negligence than design. He makes me say "as far as can be determined by the *sense of touch*," whereas, I say by the touch. It is well known to ac-

\* Heat and cold we are aware are only relative terms, but I have chosen to employ them here as separate properties for the sake of perspicuity; and when I say the teeth are susceptible of impression from cold and not from heat, I wish only to be understood, as referring to the sensation they produce, without any reference to the absolute quantity of caloric a body may contain. Thus then, a considerable degree of heat above that of the mouth, will not affect sound teeth disagreeably, whereas, when the temperature of the medium, offered to the teeth is considerably below the temperature of the teeth themselves, they will be unpleasantly affected; the reason I have elsewhere explained.

† We might notice with great propriety Dr. H.'s evident oversight of the anatomical arrangement of the ear and nose, when he demands "whether the texture of the *os tinnæ* be firmer than that of the seat of the sense of hearing, or of that of smell? Does Dr. H. mean by the seat of the sense of hearing, the petrous portion of the temporal bone, or the *portio mollis* of the auditory nerve? Does he mean by the seat of the sense of smell, the nasal and ethmoid bones, or the Schneiderian membrane? It is certainly difficult to decide this point; since if he means the *portio mollis*, it will bear no comparison with the *os tinnæ*; and if he means the bony canal in which it is encased, he must shew this part is the seat of hearing, and not the *portio mollis*. If he means both the *portio mollis*, and Schneiderian membrane, it would be easy to prove, they are much less firm in their texture than the *os tinnæ*.

coucheurs that by *the touch* is meant, the use or employment of the finger, to ascertain the state of the uterus or os tincæ or both ; therefore, I conceive there is no *Irisbism* in saying, “as far as can be determined by the touch,” or in other words, by the application of the finger, for which the *touch* is the technical term.

Dr. H. lays it down p. 105, as “an incontrovertible truth, that whatever part of our bodies is capable of being pained, may also be the seat of pleasure.” Is not bone, ligament, tendon, cellular membrane, &c. capable of being pained, yet have they ever been known to produce or yield pleasure ? Dr. H. farther says, he “will allow that the os tincæ may be freely felt by the finger, without exciting much either of pleasure or pain.” What has become now of its “extreme sensibility ?” Could any other part of the body to which the attribute of sensibility is allowed, be felt freely by the finger, without exciting much of either “pleasure or pain ?” Dr. H. endeavours indeed to reconcile this seemingly contradictory concession, by observing, “But does it follow consequently, that the friction of the penis in coition, does not excite pleasurable, and perhaps, in some, for ought I know, painful sensations ? I need not undertake to prove, that there is such a thing as specific sensibility, and that specific sensibilities are only to be aroused into activity by their appropriate objects.”

I will not pretend to deny the existence of specific sensibilities, but must say Dr. H. has not proved there exists one in the os tincæ ; I will even go farther ; I do not believe there exists any there. My reasons for thinking so are, first, pleasurable sensations appear to be excited where the “appropriate object” has not been applied ; and secondly, when applied, under the most favourable condition, as far as can be determined by existing circumstances, the specific effect has not resulted.

To prove my first objection, we need but recollect the disgraceful means sometimes employed for this purpose; with a view to a more full illustration of what is here alluded to, I will transcribe a few passages from Chambon, a modern French writer on the diseases of girls. He asks, p. 80. vol. II. “Est-ce dans les vices de l’éducation, dans la séduction des exemples dangereux, et dans la force du tempérament ou l’effet des passions, qu’il faut chercher la cause de la masturbation? Ne seroit-ce pas aussi quelquefois au concours de certaines circonstances qu’on pourroit attribuer l’origine du penchant qui porte un grand nombre de femmes à jouir d’elles-mêmes?”

After entering into an inquiry on the object of these questions, he adds, “J’ai dit que les exemples dangereux étoient une des causes les plus ordinaires de la masturbation; c’est presque toujours dans les lieux où les filles sont rassemblées en grand nombre, que cette funeste habitude se contracte.

“Quand je traiterai de la fureur utérine, je dirai quelles sont les revolutions qui se passent dans une fille d’une constitution vigoureuse, et d’un tempérament ardent; on saura mieux pourquoi les sensations qu’elle éprouve dans les parties de la génération, la forcent quelquefois à jouir d’elle-mêmes; on concevra d’avantage comment une inquiétude douloureuse qui fatigue ces organes, y fait porter la main, sans avoir même l’idée du libertinage. L’impression qui naît de ce contact devient un sentiment de plaisir, et la jouissance se consomme avant qu’on ait eu le temps de la réflexion. On juge bien que le souvenir d’un moment de délices, que l’excès de santé rappelle souvent à l’esprit, ne peut être oublié; les jouissances se multiplient, l’habitude se contracte, et les oreilles se ferment aux conseils de la sagesse.”

The extract just given will prove, that there is enjoyment without the “appropriate object,” and consequently, if pleasure or gratification be produced, by the means mentioned, it is not

essential that the parts Dr. H. supposes indispensable for their production, should come in contact.

To prove my second objection, I must again urge, that there are women who feel no pleasure from coition; yet with whom it is presumable, that the "velvet like head of the penis" and the "extremely sensible *os tincæ*" might as readily, and as reasonably come in contact, as where pleasure is the result of sexual union. That I may not appear to stand alone in this opinion, I will cite a passage from Dionis,\* (Eng. trans. p. 106) who is a warm stickler for sexual pleasure in the immediate

\* This old writer appears to have entertained very nearly the same ideas of conception that Dr. H. does; to prove this, we will run two or three parallels of the principle opinions.

## DIONIS.

By introduction is understood, the entering of the yard so erected into the mouth of the womb,† that is eager to receive it. p. 74.

These parts feel at that time a mutual tickling pleasure, *produced by rubbing* one on another. p. 74.

Then emission is ardently wished for by both parties, as the height of pleasure and full enjoyment p. 74.

The seed emitted directly into the mouth of the womb, is greedily received, and by the *contractions of the uterus* is pushed through the *tubæ fallopianæ*, to the ovarium. p. 75.

For I find that their action (the ligaments) is to draw the womb downwards, and by their elasticity, to bring it near to the yard in the act of generation, that by its internal orifice it may more conveniently receive the seed emitted. p. 34.

I have made it appear that the use of the *ligamenta rotunda*, is to bring the bottom of the womb forward towards the yard for the reception of the seed; the seed being received, the womb contracts, the seed is compressed and forced into the *tubæ fallopianæ*. p. 76.

## HARRISON.

That the venereal sensibility resides in the *os tincæ*, &c. I infer from the structure of the parts, and especially from the projection of the *collum uteri* into the vagina, where it will receive in the sexual intercourse, irritation from the soft and velvet like head of the penis, well calculated to produce the venereal orgasm; which consists in a certain excitement of the uterine system, accompanied with exquisite sensations, similar to those that take place in the male, with an unknown desire to receive the seminal stimulus. p. 432.

That an apposition may take place, appears to me intirely probable from the phenomena, &c., the *cervix uteri* becomes turgid, the *ligamenta rotunda* contract, the uterus is depressed in the pelvis, the *os tincæ* is brought nearer to the *orificium externum*, and assumes a direction favourable to an apposition. 424.

That the transmission of the semen to the ovaria, by the *proper action of the uterine system*, is as naturally the consequence of absorption, &c. p. 423.

† By mouth of the womb he means vagina, as he says, these two parts are as well fitted to each other as a sheath is to a knife, hence the mouth of the womb is called vagina.

business of conception. "And I have met with some who declared that they had no notion of that pleasure, for which others daily hazarded so much; and assured me also, that they had been got with child without being sensible of pleasure in the least."

Dr. H. p. 106, says, "I have always understood it to be a general fact, that venereal sensibility is diminished during pregnancy, and that women are less disposed to admit the embraces of the male, during this period, than at other times." Were this a fact in its fullest extent it would prove nothing for Dr. H.'s theory, since, agreeably to his own position, the os tincæ is always within the reach of the penis during pregnancy. But the fact certainly stands otherwise, and I have no hesitation in saying, I believe it almost universally obtains, where affection makes the ground work of love; or where this act is not yielded to, rather as a duty, than as a source of gratification.

This fact ought to be perfectly reconcileable to Dr. H.'s theory if it were a just one, since, if the os tincæ be the actual seat of venereal enjoyment; since we know that sensibility is increased (*cæteris paribus*) by an increased flow of blood to the part; and since, by his own quotation from Dr. Monro, Dr. H. believes this determination to take place during the venereal congress; and since, this determination can only manifest itself by a distension of vessels; we think we have a right to conclude, that whatever will increase the determination of blood, will increase the sensibility of the part to which it is determined; and as we could very easily shew that this takes place with respect to the neck of the uterus, in proportion as gestation advances; and as Dr. H. insists the os tincæ is always within the reach of the penis, we conceive, it must necessarily follow, that were the os tincæ the seat of venereal pleasure, it ought, agreeably to the Dr's. hypothesis, to augment in proportion to the advancement of gestation. Indeed, I am surprised

Dr. H. did not take advantage of the fact we have insisted on, namely, that "women in the latter months of pregnancy, who do feel pleasure from sexual intercourse, have equal enjoyment when the os tincæ is entirely obliterated, or out of the reach of the penis."

Dr. H. p. 106, acknowledges, that "if the facts upon which this position rests," could be established, it would be entitled to great weight. Let us endeavour to satisfy the Doctor on this subject. I shall try to shew from various authorities the changes which the os tincæ undergoes from pregnancy; and also attempt to prove, that it gets beyond the reach of the penis in the latter term of gestation.

"In the sixth month the neck begins to enlarge at its basis, and seems a little softer than before." Baudelocque. p. 245. Sect. 406.

"In the seventh, the neck grows still shorter, and becomes less accessible to the touch, *because it recedes from the vulva in proportion as it is developed.*" Ibid. Sect. 407.

"At the end of the eighth month its neck is almost always effaced; and its orifice so far off, that the finger can scarcely reach it, and to do it, we are obliged often to carry it as high as the sacro iliac symphysis, *right or left.*" Ibid. Sect. 409.

"In order to reach so far, we must proceed in the following manner, the woman must be *standing*," &c. &c.; he then gives a particular direction, how the os tincæ may be touched, but as it is not immediately connected with our subject, I have omitted transcribing it. Ibid. Sect. 410.

"Some accoucheurs recommend placing the woman on the bed, to touch her in the latter periods; in order, as they say, to

bring the neck of the uterus to the centre of the pelvis, by diminishing the obliquity of its fundus. But we muſt not expect any aſſiſtance from this precaution; and it is *almost always impoſſible to reach the oriſce* in that manner, and it is much better to touch her ſtanding." Seĉt. 411. From this it would appear, that the weight of the uterus was neceſſary to bring it within reach of the finger; this will be a ſufficient comment.

"In the laſt period of pregnancy, the neck of the uterus is *completely developed*." Ibid. Seĉt. 412.

"If the cavity of the uterus becomes ſtill larger after this time (the ſeventh month,) it is all at the expenſe of theſe fibres, (fibres of the neck) now become weaker. At firſt they diſtend and lengthen: then they ſeem to range themſelves by the ſide of each other; which renders the uterus *ſo thin in this part*, that the edges of its oriſce are often no thicker than two or three folds of common paper." Ibid. p. 133. Seĉt. 202.

"In the ninth month the neck of the uterus is altogether diſtended." Smellie p. 96.

"The fundus of the uterus is the part firſt diſtended, and afterwards the inferior parts in regular order; at length the cervix is obliterated, except the mere circle of the os uteri." Denman. p. 247.

"In the laſt weeks, when the cervix uteri is completely diſtended, the uterine oriſce begins to form an elliptical tube, inſtead of a fiſſure; and ſometimes, eſpecially when the parietes of the abdomen are relaxed by repeated pregnancy, diſappears entirely, and is without the reach of the finger in touching." Hamilton p. 89.

"And it becomes thinner and thinner, (the uterus) and in the laſt days is like the other parts of the womb, and is not diſtinguiſhable, but by its circumference." Dionis p. 113.

—“Tellement que quand la femme approche de son tems, il est (the mouth of the uterus) tenet applani, et presque confus avec la globe de la matrice.” Mauriceau. p. 97.

From the quotations just made, I trust Dr. H. will be convinced, that the situation, and condition I had given the os tincæ was not ideal, and made to square with a preconceived opinion.

Dr. H. p. 107, asks, “but is it a fact that the os tincæ even in the latter months of pregnancy, gets intirely out of the reach of the penis?” I have sufficiently answered this question I trust by the extracts just given. It must not however be concealed, that in some instances at the latter period of gestation, the uterus loaded with its contents is precipitated pretty low into the pelvis; this however will make nothing in favour of the hypothesis we are examining, since when it does happen the woman suffers much from any attempt at connexion. Dr. H. in the same paragraph makes the following quotation from *Monro’s anatomy*. “Moreover, says the Doctor, the cervix or neck of the womb itself, which has long remained unchanged, becomes much shorter during the last months of pregnancy, and at length forms a broad flat opening, which, towards the time of parturition, grows continually wider.” “This” says Dr. H. “I take to be the true state of the case; but can it be inferred from this passage, that the os tincæ, even in the latter periods of pregnancy, is intirely out of the reach of the penis?” certainly it cannot doctor.

I should be obliged to Dr. H. if he will point out any part of what I have written, in which I have declared, or intimated, I drew my conclusion that, the os tincæ was “entirely out of the reach of the penis” from the authority of Dr. *Monro*, or from no better or pointed authority than the quotation he has chosen to make. No; I have drawn my opinion from



accoucheurs of the first respectability, and my own observation. Dr. Monro was a good anatomist, but was no accoucheur, as the passage Dr. H. has given us clearly proves.

From the triumphant manner in which Dr. H. has ushered in the authority of Dr. Monro, he would seem to declare his certainty of victory from it, for he immediately adds, "now if the os tincæ is within reach of the penis through every stage of gestation, and I think it would not be easy to evince the contrary, the Dr.'s argument will be found to have but little force."

From the above quotation, it would appear that Dr. H. has drawn, an inference favourable (as he supposes) to his opinion, from Dr. Monro's statement of the situation of the os tincæ. Let us examine for a moment his title to it; his position, if I comprehend him (and I should be sorry to give a wrong interpretation to his meaning) will stand thus; towards the latter periods of pregnancy the os tincæ "forms a broad flat opening," which gradually becomes wider; now, as it grows broad and flat towards the later periods of gestation, it cannot be out of the reach of the penis. I will ask if this be logic? So great a stickler for logical precision as Dr. H. should have been careful how he departed from it so widely.

Has not Dr. H. asserted rather much when he says, "this assertion (pregnant women feeling pleasure) is certainly contradicted, by the experience of *all* whose marriages have been fruitful." Has Dr. H. examined any kind of proportion of those "whose marriage has been fruitful?" And because "it is pointedly and unequivocally contradicted by analogy" in the Dr.'s opinion, as "all brute females" have "an irreconcilable aversion during pregnancy, to the embraces of their males," does it follow it cannot obtain in the human female? Is she not entitled to her peculiarities as well as the brute? "In

these," says the Dr. "impregnation seems completely to suspend, for a time, the venereal sensibility."

Thus, then, Dr. H.'s analogy will not bear him out in the most material circumstances, and were we to reduce his analogy to syllogism it would or ought to stand thus; in the brute the venereal appetite recurs at stated periods, and if it becomes prolific at this time, its venereal appetite ceases; therefore the human female must obey the same law. "What kind of philosophy is this I beseech the doctor? Has he learned this mode of reasoning from Lord Bacon or from Sir Isaac Newton? I imagine not." Now, the Dr. admits, in the brute the venereal appetite is suspended, or in other words, "the brute female has an irreconcilable aversion, during gestation, to the embraces of the male;" we will go farther and say they very rarely admit them. But does this happen with the human female? Does she not admit of the embraces of the male? She certainly has no "irreconcilable aversion" from this cause to conjugal enjoyment. And does she not when she admits of this, even agreeably to the sentiment of Dr. H, feel some pleasure? Does Dr. H. not expressly declare, p. 106, he "would not be understood, as denying that women may have considerable enjoyment from sexual intercourse; as it "is perfectly agreeable to his own observations?" What analogy then subsists between the brute and human female in this particular? Not the most remote.

With respect to what Dr. H. urges in p. 109, respecting the sensibility of the os tincæ, and his comparison of it with the extremities of the fingers and point of the tongue, I could say much, but as *every* thing we know on this subject may not be fit for a discussion like the present, which is to meet the public eye, I shall pass it over in silence, pledging myself, that, should Dr. H. think the topic worthy a future consideration,

I will freely enter into it by a private correspondence, wherein I can be more explicit, and perhaps more satisfactory.

I have urged that "the os tincæ having no fixed place in the pelvis, is by no means well situated to receive the reiterated frictions of the penis;" to which Dr. H. somewhat smartly demands, "how" I would "have it placed, in order to be better situated for this purpose, than it really is." "Suppose," continues he, "its situation within the pelvis is quite uncertain, is it therefore uncertain whether it is within the vagina? No, Sir, and in the venereal congress, the penis, I suppose, has no concern any where else." From the above question and remark of Dr. H. I should conclude he had never paid much attention to the subject of midwifery, or he could certainly never have confounded so grossly the vagina with the cavity of the pelvis. He would make it appear that the vagina comprehended all the space between the os externum and the uterus, let the latter be situated where it may; a limit, not authorized by anatomy or physiology.

But were we to give Dr. H. all the latitude he requires for his boundaries, it would not serve his purpose; since it can be readily proven, that the os tincæ may be so situated, *even in the vagina*, that the "velvet-like head of the penis," cannot come in contact with it.

Dionis,\* regards as one cause of barrenness the deviation of the os tincæ from the centre of the vagina, as he, as well as Dr. H. supposed apposition necessary to fecundation; he says "or the internal orifice not placed directly against the external or vagina, but turned some way aside, so that the seed cannot be darted into it in a straight line, and consequently cannot get into the womb." And in p. 61, of the same chapter, in speaking of the cure of barrenness, he says it may be re-

\* Page 60, chap. vii.

medied when depending on this cause, "by ordering the woman, in the venereal act, to incline to the right or left side, as the mouth of the womb is turned; and if it fronts to the intestinum rectum, by using the posture of brutes."

"Si l'orifice interne de la matrice," says Mauriceau,\* est situé et regarde en dessous, ou à coté, on y pourra en quelque façon remedier, en faisant observer à la femme dans l'action du coït, une situation par laquelle la semence de l'homme puisse être éjaculée vers cet orifice."

"The uterus has no absolutely determined situation, and takes, as we may say, a new one every instant. Sometimes it is lower or higher; sometimes it is inclined towards the sacrum or towards the pubes, and at other times to one of the sides."—*Baudelocque, page 156.*

The deviations just spoken of, are not less remarkable in the impregnated uterus, than in the unimpregnated, as one of the three obliquities, almost without exception prevails. This must necessarily carry the os tinæ in a direction opposite to its fundus, consequently, cannot correspond with the axis of the vagina; this obliquity is frequently so great, that we cannot by any moderate endeavour touch the mouth of the uterus, until we have corrected the deviation, by obliging the woman to take a situation favourable to restore the fundus of the uterus to the axis of the pelvis. So extensive is this departure of the os tinæ from the axis of the pelvis, that in some cases the head of the child has escaped the os externum, covered with the uterus,† and I am tempted to believe, that some of the cases reported, of the uterus having no os tinæ, and where cutting instruments have been employed to make an artificial one for the fœtus to

\* Liv. i, page 61.

† Baudelocque, Levret, &c.

escape through, have been nothing but extreme obliquities of this organ.

From what has just been said, I think I have made it appear, first, that the os tincæ has no certain destination in the vagina, as it may be on either side or behind ; secondly, that in the impregnated state of the uterus, it is intirely out of the vagina, and at the superior strait of the pelvis ; and thirdly, that its situation in the last months of pregnancy is so variable, that nothing but determining the nature of the obliquity of the uterus could lead us to a knowledge where it should be found ; and even when that is ascertained, a great deal of trouble is frequently experienced before the finger can be made to reach it ; nay, many times I have been obliged to allow the labour to advance considerably, before the os tincæ could be touched.

Dr. H. p. 109, supposes the sole purpose of the projection of the os uteri into the vagina, is to receive the friction of the penis, and thus (I trust, he means) to afford pleasure ; I conceive the neck of the uterus is destined for much more important objects ; first, for an outlet from the uterus, secondly, as an abutment for the body and fundus when impregnated ; and thirdly, to furnish all the room the fœtus enjoys after the seventh month of pregnancy. If it were for the mere business of pleasure, why does it not constantly maintain the intention of its formation, or why does its form undergo, such gradual, invariable, and important changes ? Why was the business of sexual enjoyment intrusted to a thing so versatile in shape, and situation.

I shall not notice Dr. H.'s question respecting the intention for which the os tincæ was touched "an hundred and an hundred times," for reasons I have just assigned in another place, where nearly the same object was involved ; nor advert to his mode of getting over the shape of the os tincæ, for similar reasons.

Dr. H. p. 113, observes, "Although we are not bound to maintain that pleasure takes place in every prolific congress, yet we beg leave to notice the proofs' against this doctrine, which the Doctor has adduced from the Abbé Spallanzani. The first is taken from the bitch; in which case impregnation was effected by means of a syringe; but was this done at a time when the bitch was indifferent, or averse to the embraces of the male? no.—Spallanzani was too well enlightened on this subject, to expect he could effect impregnation at any other time, than when the bitch was hot." I would ask Dr H. what proof he has, that a bitch or any other animal cannot be impregnated but when willing to receive the male? Does he know of any experiments which puts this matter out of doubt? Has it not been rather an assumed principle, that animals can only be impregnated when in season as it is termed? Does not this being in season rather prove the disposition to venery, than the capacity for fecundation? May not an animal have the capacity without the inclination? It is, I know, generally admitted, that animals conceive but at this time, and it is ascertained, that they do become prolific at this period; it was therefore chosen by Spallanzani, as being as he supposed the most favourable condition for his new experiment; but this by no means proves, that no other time will do.

"But is it certain," asks Dr. H. a little farther on "that the stimulus of the injected semen produced no pleasurable sensations, especially when the genital organs were in a condition to receive with facility the impression of that stimulus?" I answer it is by no means certain that pleasure did not take place, nor is it material to the point in question, since it will go no way to strengthen Dr. H.'s hypothesis; for he insists, that impregnation cannot take place without the venereal orgasm, and that this cannot take place without an irritation, produced by the velvet like head of the penis, against the extremely sensible os tincæ. Dr. H. therefore makes venereal orgasm as much a

line qua non, as the male semen. Dr. H. has endeavoured to obviate this, by supposing the venereal orgasm to be always present when the animal is in heat, but as he has no proof of this, but what he derives from this being the usual season for procreation, I must not allow him this subterfuge.

Because an animal has venereal appetite, must it also have that condition termed venereal orgasm, that is so essential to fecundation, agreeably to the opinion of Dr. H. ? Does he not confound here his own distinctions ? In the human female, he makes it the result of irritation ; in the brute, either the cause or consequence of venereal appetite. If it be the cause of venereal appetite, it may exist independently of it ; therefore if this moment be seized, the animal may be impregnated without venereal appetite ; if it be the consequence of venereal appetite, the ultimate effect of venereal gratification arrives previous to its employment

Dr. H. p. 115, says “ but that pleasure accompanies a prolific intercourse of the sexes, the Doctor attempts farther to disprove from Spallanzani’s account of the manner in which newts and frogs procreate.” This is another instance of wrong quotation, or at least of wrong construction ; I have said “ that with other animals, such as the dog, frog, newt, &c. on which the ingenious and accurate Spallanzani experimented, pleasure or venereal contact were not necessary,” &c. To this Dr. H. observes, “ I have not the work itself of Spallanzani, but in Duncan’s account of it, which I have, it is stated that the ‘ embraces of the male begin, before the exclusion of the eggs commences ; and that during the discharge of the eggs, the agitation and croaking both of the male and female were very remarkable.’ It is fairly deducible then, from this testimony of Spallanzani, that sensations of some kind accompany the copulations even of frogs ; and if so, the inference Dr. Dewees would wish to be made from it, is not warrantable.”



I have wished no inference drawn whatever from this fact, as it came not within the range of my inquiry; and how Dr. H. came to choose this observation, I am at a loss to determine, unless he conceived it suited his opinion, and militated against mine; for I have expressly declared, in the experiments I had reference to, pleasure or “venereal contact, were not necessary.” Dr. H. in even Duncan’s account of Spallanzani, could easily have found to what experiments of the Italian professor, I alluded, for he says, p. 145. Comment. for 1786, “The subject of the second dissertation, is the artificial fecundation of certain animals” He then in a succinct manner, relates the Abbé’s experiments on “the terrestrial toad with red eyes, and dorsal tubercles,” in the following words, “he (Spallanzani) separated a female of this species of animal from the male, when, from the swelling of her belly, there was reason to suppose that the expulsion of the cords containing the ova would soon follow. In a few hours, after the female had been placed by herself, in a vessel full of water, the cords began to appear. When about a foot was excluded, he cut it off. He left one half in the vessel, and took out the other in order to wet it with semen he had procured from the male, that had been separated from the female, and which he had put into a watch glass. This liquor he spread with a pencil on the cord, which after this operation, was placed in a vessel of the same water as that in which the unimpregnated lay. For five days he could perceive no apparent difference between the impregnated and the unimpregnated portions. But on the sixth, he began to conceive hopes that the application of the feminal liquor had not been ineffectual. For then many of the tadpoles, in the portion which had been artificially impregnated, began to assume an elongated figure. On the seventh day, with manifest elongation, there was visible increase of bulk. On the eleventh, he perceived them moving in the amnion; and on the thirteenth, they quitted their membranes and swam about the wa-



ter. On the other hand, the unimpregnated tadpoles began to corrupt, and soon turned putrid."

I have chosen to make this extract from the work which Dr. H. acknowledges to be in his possession, to prove, he either has misunderstood me, or did not read Duncan's account with sufficient care.

Dr. H. p. 117, asks if the "following would be regarded as good logic? There are a few extraordinary cases in which impregnation is said to have been effected, without the introduction of the penis into the vagina: these cases, therefore 'furnish most unequivocally the following conclusion.' The introduction of the penis into the vagina in order to impregnation, was a useless and unnecessary provision of nature."—No, it would not, but the following I think would. Cases of impregnation have occurred where it was physically impossible, and others where the penis never was introduced into the vagina; therefore, the introduction of the penis as far as regards impregnation alone, is not *essential*. In making the introduction of the penis, not *essential* to generation, we mean not to deny the convenience, or the pleasure resulting from it; pleasure we look upon as the incentive to procreation, and it has been wisely ordered it should be imperious; it must therefore be regarded as a motive to sexual intercourse, but not an essential to impregnation.

It is pleasant to have it in our power, to obtain facts every way serving our purpose, from those who entertain sentiments very opposite to our own; I have this gratification in reading old Dionis, who seemed never to entertain a doubt, but that the semen must be projected from the penis into the uterus, that impregnation may take place. I shall therefore, the more readily transcribe it. "I have known some *maids*, who to their sorrow, have experienced the truth of what I advance;\*

\* In order to understand to what he alludes, I will give his ideas on the subject of the semen. "The moment that a man emits the two first drops of the

for not having permitted their gallants to enter their bodies, or emit, but betwixt their thighs only, lest they should prove with child, they thought themselves very secure; but afterwards found, that they were *maids with big bellies, a thing not so very hard to be conceived as some imagine.*" Dionis p. 78.

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*Case of Debility followed by an Inflammation of the Stomach and Intestines, terminating in immediate Death. Communicated by*  
J. C. ROUSSEAU, M. D.

SIR,

**A**S the case that I am going to communicate, is of rare occurrence, it will not probably be unacceptable to your readers. I will relate it as briefly as possible, so that it will, I hope, be at any rate, worth the time spent in reading it.

On the 11th of May last, I was desired to visit J. B. D. a native of the West Indies, aged about 45. He had kept his bed for a few days and complained of an extreme debility, especially in his lower extremities, which refused entirely to support his body, he falling to the ground as soon as he attempted to stand up. He complained of a loss of appetite, and was troubled now and then, with a singultus. His pulse was natural, but rather weak; the heat of his body, his coun-

feed, that is, the most subtil and spirituous parts of it, are thrown and darted into the womb, while the grosser and thicker ones move heavily along the yard, in a drivelling manner, and are lodged in the vagina." p. 78.

We need not say how chimerical this is; being acquainted with facts which forced him to admit, that neither the penis nor semen entered the vagina, and yet the woman conceived, he was obliged to have recourse to his imagination; this furnished him *with the two drops of semen that are first ejected.*

tenance, and every thing else about him was natural; in short, nothing indicated the existence of any disease. I prescribed,

R. Spir. Vitr. dulcis,  
Aquæ fontis, āā. cochlear. minim.  
Sumat ter quotid. dum singultus adsit.

This had the desired effect and he had no occasion to take more than one ounce in the course of three days.

14th. Singultus has returned; applied a small blister on the pit of the stomach; obtained relief.

16th. I was applied to by his wife, to furnish the certificate of death prescribed by the law. I could hardly believe her report, and unable to account for such a sudden death, I proposed an examination of the body, which I obtained.

The outward appearance of the body presented no marks of any violence or injury, except some frothy mucous discharge from the nose and mouth.

On opening the abdomen, the stomach and small intestines were found totally empty, exhibiting the strongest mark of inflammation, which extended to a portion of the peritonæum; the large intestines were natural and without any alteration; all the viscera perfectly sound; not the least appearance of turgescence in the blood vessels.

In the thorax, the lungs were found with a few adhesions to the pleura and pericardium, which appeared to have been of long standing, and probably the effect of former diseases of those parts. On opening the pericardium, the heart was found flaccid and extremely soft, and containing very little of

blood : on pressing the lungs, the bloody mucous discharge spoken of above, came out by the nose and mouth in a small quantity.

It is very remarkable that no blood appeared under the knife, although no precaution was taken in dissecting the pectoral muscles, which were cut in every direction, to expedite the business in opening the thorax. Indeed, from this circumstance, and the flaccidity of the heart and blood vessels, a person unacquainted with the particulars of the disease, might have supposed, that bleeding had been done ad exitum.

What I could collect from his wife was this ; he was, the day before about noon, seized with a violent chill, and shortly after, complained of a burning heat in the abdominal region ; he became delirious towards night, and died in a little time. She said she did not send for me, because she was not sensible of the danger at first, and himself expressed that he did not wish to put me to any trouble.

He had taken very little solid food for some months past, and drank more than usual of teas or sweetened water. He drank pretty freely of spirituous liquors at all times, previous to his illness ; but I do not recollect to have ever seen him intoxicated, although I had known him for some years.

I shall offer no reflexions, leaving the reader to comment upon the case after his own judgment and adopted principles.

It offers an ample field for inquiries upon the diseases that are attended with an inflammation, or preternatural redness of several parts of the animal economy. Is it then the effect of a preternatural impulse of the blood in the part affected, or owing to a derangement in that part, disposing it to receive

more of that fluid, than it is apt to do in the healthy state? I cannot but favour the last opinion, when I observe, daily, cold and heat, pleasure and pain, so opposite in their nature, produce the same effect, an increase of circulation at the surface; the florid hue and the burning in the ears, of timidity, attended with cold hands; and the emaciated state of consumption, with a burning in the extremities, &c.

J. C. ROUSSEAU.

J. R. COXE, M. D.

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*Observations on the Effects of the Nitrous Oxide, when taken into the Lungs; by JAMES WOODHOUSE, M. D. Professor of Chemistry in the University of Pennsylvania, &c.*

IN the year 1802, I prepared a large quantity of the nitrous oxide or dephlogisticated air, from the nitrate of ammoniac, made by decomposing nitre, by the sulphate of ammoniac, and by adding the nitric acid to sal ammoniac.

A great number of gentlemen, belonging to my chemical class, who intended to breath the gas, were present in the morning, when I was filling my air holders with it, and saw all the operations going forward.

In the afternoon, being alone at my laboratory, at two o'clock the air was examined, and found to be extremely impure, having made use of too great a degree of heat, in generating it.

Expecting the gentlemen at three o'clock, the impure air was thrown away, and the air holders filled with atmospheric air.

This air was breathed by a variety of persons, under the impression that it was the nitrous oxide, and the greater part of them were affected with quickness of pulse, dizziness, vertigo, tinnitus aurium, difficulty of breathing, anxiety about the breast, &c.

The following is a letter, received from one of the gentlemen.

“The nitrous oxide produced no sensible effect, for perhaps the space of a minute after I began to respire it. Soon after I was affected with a tinnitus aurium, which affected the sense of hearing, in the same manner as water, in a state immediately preceding ebullition does. At the same time I had a sensation similar to that produced by swinging; afterwards a difficulty of breathing gradually came on, which at length necessitated me to discontinue the respiration of the air. The difficulty of breathing and the tinnitus then soon subsided, but the peculiar sensation in my breast, continued some time longer, which was succeeded by slight nausea, which continued six or eight hours.”

A short account of the effects of the atmospheric air, was sent to Dr. Mitchell of New York, who published it in the fifth volume of the Medical Repository.

For many years after this, not finding the experiments of Mr. Humphrey Davy on this subject, confirmed by other chemists, I believed that the influence of the imagination, caused all the effects ascribed to the nitrous oxide.

In the winter of 1806, having prepared a quantity of this gas, extremely pure, from the nitrate of ammoniac, made by a direct combination of the nitric acid and the carbonate of ammoniac; two quarts of it were administered to Mr. Henry

Latrobe, fourteen years of age, who breathed it in a very fair manner. In a minute he was most violently affected. He walked up and down the laboratory with a quick step, elevating his legs, then suddenly throwing them down on the earth. He rubbed his hands rapidly over each other, and laughed immoderately and convulsively. The tears rolled down his cheeks in large drops, and he frothed at the mouth.

Witnessing these effects, and knowing the impossibility of counterfeiting such symptoms, I immediately resolved to try the effects of the gas, on other persons.

Doses of two and four quarts, were always administered.

Mr. J. D. M'Clean upon breathing the gas, fainted and recovered in about three minutes.

Mr. George Thornton looked wild, jumped over a high railing, and the effect suddenly ceased.

Mr. Martin raised his hands over his head, and jumped about the room.

Mr. Pope placed his arms a-kimbo, and surveyed the audience with great contempt.

Mr. William Barton was very much deranged. He run about the laboratory, bellowed like a mad bull, and struck at every person near him. A week after, the gas being administered to him a second time, produced the same effect. He felt an increase of strength, after recovering from the effects of the air.

It was with great difficulty, I could remove the mouth piece of the bladder, from his mouth.

Mr. N. S. Allifon fainted, but recovered in a few minutes. Upon breathing the air seven days afterwards, the same effect was produced.

Mr. Thomas Prioleau exclaimed, "I am in heaven, ye gods, stars, comets, meteors, Mahomet's a jackass, the Elysian fields are hell compared with this," and then fainted.

Mr. Robert Patterson was affected with violent laughter.

Mr. Samuel Jackson in the same manner.

Mr. Peter Curtis laughed very heartily.

A week after, having a large air holder, filled with atmospheric air, standing along side of two others containing nitrous oxide, the atmospheric air was given to him, but it produced no effect.

Mr Gerard Snowden fainted, but soon recovered.

Mr. William Handy laughed and fainted.

Mr. William Tyler fainted, and recovered in four minutes. Seven days after, breathing the air a second time, the same effect was produced.

Mr. Cornelius Dupont laughed and fainted.

Baron John de Bretton experienced pleasurable sensations.

Mr. Benjamin Kugler laughed ; upon giving him atmospheric air a week afterwards, he was not affected.

Mr. Thomas Lewis was much enraged. He caught me by the collar, pulled at my cravat, tore my coat, ran about the room, and struck at every person near him.



Mr. Evans breathed atmospheric air ; it produced no effect.

Mr. Wheaton after taking four quarts of the nitrous oxide into his lungs, was beginning to be affected : he cried out in a very rapid manner, "give me another bottle, give me another bottle."

The gas was tried upon fifteen other persons, without producing any effect. Some of them breathed it in a very fair manner : others were much frightened, and mixed it with the air of the atmosphere.

I am now perfectly convinced, the gas produces all the effects ascribed to it, by the justly celebrated Mr. Humphrey Davy, who first took it into his lungs ; and I am happy in having this opportunity, of confirming his experiments.

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*An Account of the Influenza, as it appeared in Philadelphia ; by*  
JOHN C. OTTO, M. D. *of Philadelphia.*

THE weather, during the summer has been uncommonly mild, the atmosphere damp and the rains very frequent ; but I have not learned by precise measurement the number of inches that fell. Nothing unusual was observed in the appearance of our diseases, except that in July, an ophthalmia affected so many persons as to constitute quite an epidemic ; it yielded however to the remedies that are employed for that complaint. The public papers conveyed to us the information that towards the latter end of that month the influenza was very prevalent in the city of New-York ; and in the beginning of August it com-

menced its career with us. Some imagined that the ophthalmia was the form under which this epidemic had attacked some persons, or that those who had been previously affected in that manner would be exempt from the influenza, but experience has shewn the fallacy of the supposition. It began in ordinary with the usual symptoms of a common catarrh, such as chilliness, lassitude, hoarseness, cough, fulness and pain in the head and chest, followed by fever of an inflammatory type. The chilliness often alternated with flushes of heat, and was accompanied by a desire to approach the fire or to have an unusual quantity of clothes on the bed ; this state when the disease was mild, often continued during its course ; but in the more violent attacks it merely ushered in the other symptoms. The head was very uniformly affected with pain, and sometimes to a great degree ; at others it principally occurred during coughing ; the seat of it was various, but it was most ordinarily confined to the frontal sinuses : delirium attended a few cases, and in one instance that came under my notice, a complete derangement of mind took place. Drowsiness was rare, and was principally confined to children : the reverse was much more frequent, many persons being troubled with a constant inquietude and inability to sleep, vertigo sometimes occurred, and I think that women were rather more subject to it than men. Sneezing, and a considerable discharge from the nose were very common ; but these symptoms were not so frequent as I have seen them, when the influenza prevailed formerly. In these cases, when the secretion was profuse, the head was relieved, and when the principal force of the disease fell on it, and not on the chest, it terminated soonest. The eyes were sometimes brilliant, red, and watery, occasionally shedding tears as is usual in the measles ; and when much affected they were extremely sensible to the light. The tongue was generally moist and white, the countenance sometimes flushed, and the face a little swelled. Hoarseness was a frequent symptom, but that which was by

far the most distressing and uniform attendant, was the cough, which often kept the patient in a state of constant uneasiness; by its repetition it prevented sleep and renewed the pain in the head and chest. It was often moist even in the commencement of the disease, without expectoration; towards the decline, the discharge from the lungs of an opaque mucus, that often took place, gave relief: in some it was dry and very painful, as happens in the beginning of pleurisy. During the evening and night, its frequency, when not checked by opiates was the greatest, although the position of the body was not altered; it being always the most severe in a state of recumbency: in the advanced stage of violent cases, when it was safe to administer anodynes, it became most troublesome early in the morning, until the secretion in the lungs during the night had been discharged. In some it was occasionally convulsive, as in whooping-cough, and like it, was sometimes followed by a discharge of the contents of the stomach, more especially in children. This distressing symptom often remained weeks after the violence of the disease was removed, and upon slight exposure, was apt to be increased. The breast in many cases was extremely sore, and patients often complained that it was "as raw as beef," and endeavoured to repress coughing from the acute sensation it occasioned, being different from the lancinating pains in the sides and shoulders, that occurred in those cases where the disease assumed the more complete form of pneumonia. The breathing was often laborious, appearing in children, in some instances like what occurs in croup; in grown people, sometimes painful, as takes place in pleurisy; while in the more advanced, it was frequently attended with that difficulty that exists in pneumonia notha, the form in which it occasionally attacked them, especially when it was fatal. The limbs and back were sometimes as much affected with pain, as is usual in our autumnal fevers; and a distressing cold clammy sweat often occurred in the beginning of the dis-

case, without producing the least mitigation, although it was frequently profuse.

The fever evidently remitted, was generally worse in the evening and night, and often was attended with a gentle perspiration in the morning. The pulse was generally frequent, full, and sometimes hard. The skin was often dry; nausea sometimes occurred, and a very general disinclination from food. I saw but one case in which there was any diarrhœa, and that was in a lady far advanced in pregnancy; a costiveness frequently attended; in two instances, an eruption like a rash, was diffused over the whole person. I visited a lady, who was in a gentle perspiration with comfortable feelings, but supposing the lamp would go out, arose to trim it; upon getting into bed, she found the perspiration checked, and was confined several hours to a sitting posture, being unable to move without experiencing the most excruciating pain in her side and shoulder. The circumstance took place in two others without this exposure. Although the influenza generally assumed nearly the same set of symptoms in the different subjects, varying only in grade; it was diffused too extensively not to admit of some variety of appearance, according to the various predispositions of those attacked. Persons who were liable to gout in the head, asthma, or affections of the breast, had a renewal of their former feelings, upon being affected by the influenza; and I attended an adult who had once been on the verge of the grave from a violent spasmodic affection of the trachea, that was brought nearly to the same condition by this complaint. From indiscretion, and a change of the temperature of the atmosphere about the height of the disease, some persons relapsed, and had not only a return of the cough, but had their lives endangered.

No particular description of persons or professions appeared to be especially affected; but children commonly had it milder,

and were more liable to be exempt ; I however visited some in early infancy, and others in the much dreaded form of cynanchæ trachealis. In many instances it attacked people confined to their houses, and even beds, with other complaints.

It commenced in the beginning of August ; the new cases diminished rapidly after the third week, and few occurred after the fifth ; and from the most correct information I could collect, I am induced to believe that not more than one seventh, if so large a portion, of the population escaped.

The duration of the disease varied much with its force, and the medical treatment ; in general it lasted two or three days, and very rarely was prolonged to a week, unless it was attended by a very unusual determination to the brain or uncommonly violent symptoms of pleurisy ; but after it had apparently terminated, some unpleasant feelings, and discharge from the nose, or cough, would continue several days longer ; and this last symptom sometimes two or three weeks. Thousands were so slightly affected as not to be in the least interrupted in their usual occupations ; and the generality of persons required very little medical treatment and received none, except occasionally a few domestic remedies, the most popular of which were castor oil, and producing perspiration by taking some warm tea and bathing the feet. A rigid abstinence from animal food, soups, and stimulating drinks : warm clothing, a brisk cathartic or two, and confinement to the house, which last direction was rarely submitted to, by those who were able to go abroad, were all that were requisite in most cases. In others, the disease assumed a more serious aspect, and required the prompt use of active remedies.

Bleeding was ordered to a majority of the persons, whom I was expressly called to visit, and sometimes the operation was repeated two or three times, and in a very few cases to four or

five, where violent symptoms of pleurisy or great determination to the brain occurred. In addition to venesection, cupping was employed with great advantage to an aged man, who was threatened with apoplexy. This remedy was used as long as the full and active pulse and symptoms of inflammation remained unsubdued; in the aged, when pneumonia not so threatened, bleeding was required, although the pulse was often but little altered. The loss of blood was very generally followed by a diminution of the fever, and great relief in the head and breast; in the advanced state of pregnancy its good effects were uniformly discovered in a short time, very often before the operation was finished. During the first days of the complaint, the blood was rarely fizy, but in violent cases, and after the disease had continued some time, it pretty frequently exhibited this appearance, and was occasionally much cupped, especially where bleeding had been improperly delayed. The serum was generally yellow, and the crassamentum dense, often sinking to the bottom of the vessel.

PURGES, were found of great service in keeping down the phlogistic diathesis of the system; and where the head rather than the breast was affected, they were of peculiar benefit. I have known many persons with considerable fever, and so unwell as to be induced to confine themselves to their beds, to be completely cured in six and thirty hours by the brisk operation of a purge six or eight times. The discharges were often of a dark colour.

EMETICS. When the disease was not violent, and affected the breast chiefly, or after the inflammatory action was principally subdued, and the cough was troublesome and moist, and the breast oppressed with phlegm, I found emetics to be of great advantage. Their operation when plentiful, was generally followed by much relief of pain, and in breathing; and where advanced pregnancy did not forbid their use, they were admi-

ministered pretty frequently, especially to young children, whose symptoms often indicated a strong tendency to cynanche trachealis.

ANTIMONIAL DIAPHORETICS, were likewise given towards the decline of the disease in violent cases, and sometimes in the beginning of mild ones; and after a general warm perspiration commenced, the feelings were rendered more comfortable, and the fever diminished. A profuse sweat for eight or ten hours in the forming state of the complaint, often completely removed it.

BLISTERS, were not employed except where the disease assumed the form of pleurisy, or where the determination to the head was very great; in these cases they answered the expectation formed of them. When applied to the side, they relieved pain and promoted expectoration.

OPIATES, appeared to be indispensable to allay the coughing, and procure sleep; and were freely administered at bed-time, whenever the inflammatory symptoms were sufficiently subdued to admit of them. A troublesome cough with a trifling expectoration, frequently remained after the other symptoms had disappeared, for which I gave with evident advantage, a table-spoonful of the following mixture every three or four hours, varying the antimonial wine and tincture of opium according to circumstances.

R. Suc. glycyrr.	ʒiss.
Gum. Arab.	ʒiss.
Vin. Antimon.	ʒiij.
Tinct. opii	gtt. xxx.
Aq. bullient.	ʒvi M.

Demulcent drinks, as flaxseed tea sweetened with honey or sugar, to which lime juice was added; barley water or bran



tea, were administered plentifully, and were ordered to be drank gently warm; for cold drinks as is usual in pectoral affections excited coughing: the most rigid abstinence was enjoined.

In no instance did I visit a patient to whom I conceived it necessary to administer tonics.

The mortality was exceedingly small compared with the number attacked, and was principally confined to aged people, invalids, and persons of consumptive habits; but from the parts principally affected, it is presumable its consequences will not be so slight, and that a foundation will be laid in many of a serious disease in the chest.

Many, who had suffered neither long nor severely by the influenza, complained of an unaccountable weakness, even when no depleting remedies had been used, and the disease been permitted to take its course; the same was observed in others who had been subjected to medical treatment. The appetite often remained impaired, days, and sometimes weeks, after the disease had entirely left them; and convalescence was rarely distinguished by that strong desire for food that accompanies recovery from our autumnal fevers.

The ancients, referred the cause of epidemics to a certain something in the atmosphere, which is not discoverable by our senses, but which, like the great cause of all things, shews its existence and agency by its effects. The more moderns, endeavoured to be more precise, and ascribed it to the conjunction of planets, or to mineral exhalations from the bowels of the earth; this was merely substituting for one conjecture, another equally vague. Nor have the physicians of the present day, with all the aid of modern chemistry, been able to develop principles that have led to correct opinions of the cause of this and many other epidemics; and I regret it is not in my



power to furnish any thing more certain. It appears from the various histories of the influenza upon record, that it has no connexion with the sensible qualities of the air, for it has occurred in every climate, season of the year, and temperature of weather, and without being materially altered in its symptoms. No disease in the annals of medicine has spread so widely and rapidly as this, for it has often visited Europe and America, and most probably diffused itself over the whole world. Two opinions divide the medical profession with respect to its propagation; some, from the rapid manner in which it spreads, rarely continuing in any city however large beyond four or five weeks, have referred its diffusion to some unknown epidemical constitution of the air, which produces the disease in all attacked; while others, ascribe it to a secreted animal poison that emanates from the sick, and through the medium of the atmosphere creates a similar disorder in the well. This opinion I am inclined to embrace. Philadelphia was attacked very generally, many days before there was a proportional number of sick in the adjacent counties, more particularly in those districts that had but little communication with it, although they must have breathed the same atmosphere. Large cities appear to be points from which it spreads in every direction, regulated in some measure by the intercourse with them; and the towns on the great post roads that are much travelled, suffered earlier than the surrounding country. The rapidity with which the disease spreads, arises from the mild form in which it generally appears, permitting persons to attend to their usual occupations, while there is no dread of it, to prevent the usual communication between the people. These facts however, are not sufficiently clearly established to remove all doubts from my mind upon this subject.

Intermittents and dysenteries both in the city and country, followed closely the footsteps of the influenza.

*Philadelphia, September, 16th, 1807.*

*Case of Chorea Sancti Viti; communicated by* DR. MAXWELL  
M<sup>r</sup> DOWELL.

*York, August 1st, 1807.*

SIR,

**Y**OUR “observations on Chorea Sancti Viti, *with a new theory of the disease.*” Medical Repository, Hex. 2. Vol. II. has brought upon you a severe, and in my opinion, unmerited philippic from one of our brethren across the water. It could not have been Dr. Patterson’s enmity to *theory* in general, that occasioned his severe remarks against you, for you know, he also *theorizes* on the diseases, and I am not able discover an expression in your observations, on the case stated by him, that ought to have caused the Doctor to dip his pen in gall.

Theory and practice, or *experience*, in medical science have had their respective advocates. They ought to be in *reality* what the French republic *pretended* to be, “one and indivisible.” “To think,” said the ingenious Dr. Darwin, “is to theorize; and happy therefore is the patient, whose physician possesses the best theory.” That mode of theorizing, therefore, which has facts for its foundation, and which in its progress, keeps within view of collateral facts for its support and confirmation ought to be diligently cultivated. Without *theory* there must be an end of improvement in any science.

Impressed with an opinion that every fact ought to be carefully noted, which can, even in a small degree, elucidate the pathology of any disease, I send you the following case of chorea. It is entirely left to your own judgment, to bestow upon the case what notice you may think it merits. It goes

as far, perhaps, as any single case can go, to support your *theory*, that *Chorea* is, “merely a symptomatic affection,” and, “ought not to be viewed in the light of an idiopathic disease.”

DAVID SHETRONE, best known in this country by the name of “Lame Davy,” when three years of age was seized with a disease in his left thigh, (probably a white swelling of the knee,) which from that time has deprived him of the use of the limb. His leg is flexed so as to form an acute angle on his thigh. When eight years of age, he was hopping about his father in the fall season, who was engaged in killing his winter’s meat. He picked up the butcher’s-knife, and almost immediately tripped and fell with his head upon its point. The point of the knife struck the *right* temporal bone about half an inch posterior to the external angle of the eye. The cicatrix of the wound is more than an inch in length. A moderate pressure with my finger, on the eschar, discovered a considerable oblong depression of the bone. He told me he arose after the fall, and hopped to the door of the house, where he instantly fell,—that his friends carried him to a bed, and that the only sign of life discoverable, for three days, was, that his body retained its natural warmth. This account he received from his parents. He informed me, that as soon as he had so far recovered as to feel a disposition to move his limbs, he found he could not move his *left* arm, which continued to be completely paralysed for three weeks. It then began to perform those involuntary gesticulations, which nosologists have denominated *chorea*, and to this day continues to be entirely disobedient to the will. He is not able to recollect that he felt any head-ach, either immediately after the accident, or at any subsequent period. His eyes retain their natural appearance. But is not cause and effect so palpably connected in this case, that it is *impossible* to assign any other cause, for the *three weeks* paralysis of the left arm and its consequent *chorea*, than the injury which the brain sustained from the butcher’s-knife on the right temporal bone?

Davy's exertions to procure a livelihood, under so many personal disadvantages, indicate a considerable independence of mind ; for although he has only the use of *one* arm and *one* leg, he not only supported himself, by his own industry, but some years ago he married, and his family now consists of a wife and five children. The oldest of his children is about fourteen years of age. When the agent, for the proprietors, was selling the ground about this place, that had been considered a common ; Davy purchased a lot adjoining the town, on which he built a house, the logs of which he *hewed* himself. He *hewed* and *mortised* the posts, and prepared the rails for a fence to his lot. He was able, a few years ago, to cut a *cord* of wood in a *day* "from the stump ;"\* and he told me this evening, that he thought he could still cut as much wood in the same time. I have lately seen him in the act of splitting a knotty piece of wood with his axe. His sound leg and arm are on the same side of his body. He kept his centre of gravity in its proper place by an unaided muscular exertion, and his accurately directed blows, appeared very little inferior, either in force or effect, to those of most men, who enjoy a perfect use of all their limbs. The strokes of his axe, it is true, were not in as quick succession as if it had been wielded by two arms.

In travelling, Davy makes use of a crutch, and while he is thus exercising his locomotive powers, his *left* arm is constantly performing a variety of curious gesticulations. Sometimes the arm is suddenly elevated nearly to an horizontal position, from which, it is as suddenly removed, by a contrary muscular action, while the flexors, extensors, adductors and abductors, are forcing his fingers into a variety of motions, in rapid succession. When his body is at rest, the choreatic arm is also at rest ; but he says, even when he is sitting quietly at home, if a stranger steps into his house, his arm becomes af-

\* An expression used by farmers to denote the felling of trees, as well as cutting them into cord wood.

fects with chorea, or to use his own language "he won't be quiet." Does it not prove that a diseased brain is the *cause* of chorea, when we find that a circumstance which disturbs the mind, brings on a *repetition* of that singular "symptomatic affection?"

Davy says he is now in the fiftieth year of his age; and whether his chorea at this time, after a lapse of *forty-two* years, is the effect of habit or a continued *chronic* state of hydrocephalus, I will leave to abler pathologists to determine.

If an opportunity should offer, for examining the state of Davy's brain, I will most gladly embrace it.

With due respect,

I remain your humble servant,

MAXWELL M'DOWELL.

JOHN REDMAN COXE, M. D.

*Account of several Cases of Chorea, Sancti Viti; by DR.  
T. W. SMITH.*

*Loudoun County, (Vir.) September 1st, 1807.*

DEAR SIR,

**A**S few cases of the Chorea Sancti Viti have hitherto appeared in print, and as it is a disease which is evidently gaining ground in this county, it may not be deemed inutile to transmit four genuine cases, which occurred in my practice within two years past. They come therefore to implore your perusal, and the honour of a place in the Museum; or an exit in oblivion, according to their merit. The first of these cases occurred in Mr. William M'Donald, about 22 years of age,

very athletic constitution, who was attacked on the 4th September, 1805, with a violent bilious fever; an intermission of which, however, was soon procured. I then advised barks, solution of arsenic, serpent. &c. to prevent a return of the fever which I told him for certain, would ensue in a day or two, unless he took more medicine. He nevertheless obstinately opposed this admonition, and with his parents declared himself well. I then left him with reluctance; a quotidian intermittent followed for six weeks, when each paroxysm began to develop symptoms of chorea, while the intervals appeared for sometime free from complaint as usual. This transfer of excitement from the arterial to the nervous system was very gradual and progressive; the symptoms of chorea for some time appearing only in preternatural convulsions of the extremities, during the cold stage, and which also disappeared therewith on the approach of fever. Next the usual period of the cold stage was chiefly occupied in convulsions of this kind, with very little subsequent fever: and at length the cold as well as hot stages became entirely absorbed in convulsive paroxysms, which soon assumed the type of double quotidian, and lasted from two to three hours. The action of every muscle of the body, appeared now to associate in the production of the most violent general convulsions. The trunk was wreathed to and fro, the countenance much distorted, sometimes an impetuous motion of the eye-lids and lower jaw. A rotatory motion of the head was frequently very alarming, and denounced a dislocation of the cervical vertebræ. Alternately one or both arms or legs, and sometimes all at once were agitated in the most violent manner; and which no manual force could restrain. A short, quick, and interrupted respiration, with a loud and peculiar moaning, always accompanying a nodding motion of the head, was very distressing to spectators, and frequently alternated with the convulsions of the limbs. A finger, a hand, a foot, the tongue, or eye-lids, &c. were at times separately and alter-

nately affected, and these latter symptoms generally preceded and terminated the paroxysms. During these fits he was mostly confined to the bed by assistants, appeared sensible as usual, and when spoken to, would utter inarticulate answers. His pulse was nearly natural, being increased only by the violent action of the muscles on the blood vessels. With these symptoms the paroxysms returned with considerable regularity for a fortnight, when their periods were interrupted by more frequent and promiscuous returns thereof. Their violence, however, was proportionably diminished, yet scarce an hour in the day, that did not render fresh monuments of distress. He, though much debilitated, could now walk or ride about daily, with a companion to prevent him from falling in every exacerbation of his symptoms. If seized in his seat, he would sometimes spring instantly to his feet, and leap, run, and dance, with inexpressible agility for some time, and frequently to a considerable distance: nay, often until his strength was exhausted by excess of muscular action, and he sunk to the ground under indirect debility. This had been his situation three months, when I was again consulted for his relief. During three weeks of which period, he was bled and purged every two or three days, by a quack, without effect. Having never before seen the chorea sancti viti, so strikingly delineated, my curiosity frequently led me to visit him, in order to mark its progress, although at that time I was not permitted to prescribe. His debility however, at length confining him chiefly within doors, he requested to know, "if any thing could be done for his relief," as he said, he "was fast inclining towards the grave." I answered, with equal disparity, that I hoped there could. I then gave him an emetic-cathartic, and succeeded it by boluses of ferri rubigo, and the gums. Barks in a strong decoction of serpent. virg. and Fowler's solution; blisters on the head and extremities, and a nourishing diet. Slight electric shocks were frequently sent through his head and various other parts, and he was insulated. The latter, if



had recourse to in the commencement of a paroxysm, would sometimes check it instantly. Under this treatment his strength and appetite returned slowly, his convulsions became gradually less frequent, and fits more distinct. In two months his health was perfectly restored, and his fits returned only once or twice a week. I gave him the nitric acid for eight or ten days longer, which put an end to his symptoms, and which he said, was of infinite service. He remained entirely free from complaint several weeks, when harvest commenced, and excessive labour, with the debilitating power of heat and spirits, brought on a relapse. His convulsions however, were much less frequent, and more trivial than formerly; and which at length chiefly went off without any medicine. It has now been thirteen months since his last attack, and he has never within the last six months experienced any return, except at church, under the influence of the pulpit, from whence he has generally to be removed. His fits are even at that time, mild and transient.

The other three cases above-mentioned were females, viz. Miss E. Harper, Miss C. Grimes, between 14 and 15 years of age, and Miss Tolbert aged 8. The symptoms in all these cases were nearly similar, and briefly as follows. A constant motion of the head and mouth, a lolling out the tongue, inarticulate speech, unsteady walk, frequently sideways, and dragging one leg after them; difficulty in getting food and drink into their mouths, dejected countenance, some degree of idiotism. Incessant pulling at their clothes and fingers, a slight fever with debility and pains in various parts, or, in short, slight and constant convulsive motions of all the muscles of voluntary action through the day, and in one, all night. (These patients were all cured in a few weeks without relapse,) by the treatment generally recommended in such cases viz. one venesection, an emet. cathart. electricity, cold bath, bark wine, steel with the gums, blisters, anodines at night, equitation, &c.



Miss Grimes had a menstrual discharge for the first time while under my care.

From the cause of the first of these cases, being evidently such as offered the utmost violence to nature, and from the period of life at which the others occurred, can we for a moment hesitate to assign (with Dr's. Cullen and Townsend,) debility, with a morbid accumulation of irritability, as the predisposing cause of chorea? This much, however, is certain, that debility preceded and accompanied the convulsions in every case before mentioned. In a constitution like M'Donald's, which had perhaps, never before been pervaded by disease, and which was under the almost incessant stimulus of exercise, a vast secretion and expenditure of excitability, (or sensorial power,) it is evident must always be going on. If the excitement then became suddenly exhausted by excessive action, or more gradually, from the abstraction of stimuli by the causes which induce direct debility, and the excitability still accumulating, what method must nature pursue, in order to reduce the irksomeness of a continued attitude of all the muscles subservient to stimuli; and thereby exhaust the superfluous excitability? Is it not an established maxim of the animal economy, that this shall be accomplished by some motion of the moving fibres which may be in part voluntary, and in part also involuntary? In health, every person knows how soon his body or any member thereof, becomes fatigued with a continued attitude, and how easily relieved by motion. This is not the elasticity of the extended muscles exerting themselves for relaxation, so much as absence of the stimulus of exercise, and accumulation of irritability. If therefore, a degree of debility occurs in the system which excludes almost intirely the stimulus of locomotion, and at the same time not counteracted by artificial stimuli; what must be the effect of the consequent vast accumulation of excitability? In the first place, a slight fever frequently takes place, but which, without other exciting causes, is for

the most part very inconsiderable, and the superabundance of excitability is expended on the muscles, and produce those involuntary twitchings, or convulsions, which characterize the chorea sancti viti. Is not this theory sanctioned by the remedies which we find most successful in the cure, being tonic and stimulant? And will not a well regulated course of the same, always prove effectual, when debility with morbid irritability alone is the cause? Is it not further evinced by the accession of paroxysms, when exposed to the sedative effects of grief and fear, as in M'Donald at church? Electricity, by speedily consuming this susceptibility, prevented a paroxysm after symptoms commenced. When a morbid increase of irritability appears (chiefly in the nervous system,) as in typhus, do we not hear our patients incessantly complaining of fatigue, and as frequently in vain altering their position in search of rest. Ask such patients if their tossings and wrestlings are voluntary, they will reply in the negative. Solicit them to lie still, they will tell you they could as easily restrain the action of the sanguiferous system, or evade an epileptic fit, as to remain any length of time quiescent. How much more impetuous then, must be the action of the muscles under the influence of the cause, increased as before mentioned.

That sublime agent of nature which gives birth to organization, and which still continues to govern and protect the laws of animals and vegetables in their growth and functions through life: that power, by which the foetus in utero is so constantly compelled to alter the position of its limbs, and thereby not only to consolidate its fibres, but also in some measure to associate those actions with volition, and prepare it the better to enter on the wide stage of complicated necessity: That divine monitor in the female uterus, which, excited by the stimulus of distention, propels the living foetus when mature, and before it materially injures the mother: That vis-

infita, or supreme monarch, residing in the muscular fibres, which being awakened by the stimulus of the reflux blood into the chambers of the heart, restores vivacity and force to the quiescent system, as in syncope; and which being again acted upon by artificial stimuli, restores life in suspended animation, as in those drowned, killed by electricity, &c. That godly faculty of the human soul, which being accumulated in the mind of the absent husband or wife, by augmenting their affections, and happiness in meeting; in a great measure compensates for the pain of absence:—This divine blessing, susceptibility, or efficiency in the animal economy, I say, when in excess, frequently becomes the cause of disease.

Believe me dear Sir, to be

With profound esteem,

Your humble servant,

THOMAS W. SMITH.

DR. JOHN REDMAN COXE.

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*Observations on DR. GOUGH's Remarks, on DR. WENDELL's Case of Worms in the Bladder. By DR. M. WENDELL.*

*New-York, September 8th, 1806.*

SIR,

**D**ISCLAIMING every thing like controversy, when put in competition with statements of greater moment: I should not at this time have obtruded myself on your patience, or on the good sense of your numerous readers, had not some "cursorious remarks" by Dr. Gough of Charleston, on the case of worms in the urinary bladder, communicated to you last winter, met my reading.

My intentions were primarily, to have passed over these remarks in silence; but reflecting that silence would, in this in-

stance at least, be construed into an acquiescence with the Doctor's opinions, of my "being mistaken," &c. I deemed it prudent to make a few observations in reply to them.

"That truth ought always to be the object of our researches," I cordially assent to ; but am of opinion, that facts with rational deductions, will have a greater tendency to establish principles in science, than any "collision of sentiment" whatever.

The whole difficulty of conception relative to my explaining the manner those substances, got into the bladder, rests on the supposition, that no "*living* body could perforate the colon, and so sensible an organ as the kidney, without inducing consequences fatal to the patient, and that perhaps immediately." What extraordinarily deleterious properties are attachable to *living* substances, that could render them so highly and so immediately fatal, in similar cases, where *inert* substances, as pins, needles, hairs, &c. have been admitted with impunity, and expelled, by the salutary efforts of nature, without adding to the bills of mortality ? Indeed, are not facts familiar, of "hard and sharp" substances having been swallowed, and after some time penetrating the coats of the stomach, an organ, the most sensible of the abdominal viscera, and eventually perforating the paries abdominis, without any fatal consequences ensuing ?

I cannot but admire the ingenuity of Dr. G. as displayed in his definition of the substances in question :—"They were *not worms*, from the consideration of their not being possessed of *life*." I will readily grant that they were not ipso facto, and de jure, worms, or animated substances, *after* their exit from the catheter ; but the probability is, that they were so, *previous* to their entrance ; and from the *fact* of their passing through the small holes of a common sized catheter, the inference is, that in consequence of the "close squeezing" they experienced, their vital energies having doubtless been exhausted by their continuance in an acrimonious, and non-nutritious liquor, had become at once extinct by this operation ?

By a reference to the case, the Doctor will find, that his remarks on the acrimonious, and non-nutritious qualities of the urine, are anticipated: but could not these worms by their irritation in the bladder, have caused an increased secretion of its lining mucus, on which they might have supported themselves, however feeble for a short time?

It is to be hoped the Doctor will not attach *credulity* to me, when I declare, that so incredulous was I, as not even to credit their being worms, till reduced to *unerring certainty*, by the close ocular examination of a number of respectable medical gentleman, and in two instances by *glasses*. It is presumable these gentlemen, having no interest at stake, could not be biased in their opinions.

Dr. G. after apologizing for offering an opinion on the subject, adduces one of the celebrated Cullen. After going through with the "extract from his first lines," he exultingly exclaims, here "then we have a solution of this case at least;" not quite so easily solved, Sir,—for, from the very extract itself will I at least refute your inference. These substances "take the form of these vessels (ureters;)" admitting the diameter of the ureters to be "the larger the nearer the kidney,"—still it cannot be denied "that their transverse section is always circular;" now by a reference to the case of Mr D. it will be perceived, that *one* of these worms "was flat and depressed" at one end, with an "elevated circle" near the other. *Quere?* What part of the ureter could this have derived its *form* from?

The Doctor may rest assured, that so far from Mr. D. having ever had *hematuria*, or being occasionally afflicted with *gravel*, that he never had to the best of his recollection, previous to this attack, experienced any symptoms indicative of either: And further, that *since* it, he has enjoyed an uninterrupted health; unattended by any affections, naturally consequent on either hematuria or gravel.

That your various efforts to enhance American Medical Literature, may meet their just rewards, not only in the *liberality of criticism*, but also in the *more evincive liberality of subscription*, is the sincere wish of

Your humble servant,

MATTHEW WENDELL.

J. R. COXE, M. D.

*Successful Exhibition of Arsenic in a Cancerous Affection of the Tongue ; with Remarks on the Superiority of Mercury, exhibited in the State of Gas. By DR. P. W. LITTLE.*

*Mercersburg, June 12th, 1807.*

YOU requested, my dear sir, I would send some communications for your valuable publication, the Medical Museum. With that view, I send the following case of *cancer* cured by Fowler's arsenical solution, after two unsuccessful attempts to extirpate it with the scalpel ; and, if my feeble efforts can, in any manner, advance our extensive and complex science, they will be exerted with that view ; submitting with all deference to your better judgment, such cases as may appear interesting either in their nature or cure

COLONEL A —, near this place, had unfortunately, a cancerous excrescence situated on the under part of his tongue, which was dissected away, by my friend Dr M. C — at two different times ; and still having a bad appearance, took by the Doctor's advice the saturated solution of arsenic. After continuing the use of it for some time, the ulcer on his tongue became quite well, and what was singular, several *corns* as they are usually termed, which were on his feet for a number of years, intirely disappeared.

The above case appears pregnant with useful instructions, and it is consoling to reflect, that we are possessed of a medicine which promises to be useful not only in removing those troublesome and painful excrescences; but of such general utility, when judiciously administered, as your valuable essay, read before the Medical Lyceum, upon the use of arsenic, evinces. Physicians ought ever to bear in mind the invaluable maxim of our late illustrious Lenvier.\* “Poisons in small doses are our best medicines, and our best medicines in large doses are poisons.”

I have lately derived the most happy and salutary effects from the use of mercurial water in affections of the throat when inhaled. It acts as a discutient and expectorant.

I have at present, a patient with phthisis pulmonalis, brought on by a protracted pneumonia, in a convalescent state by the use of mercury. My mode of exhibiting mercury in those affections, is, in a state of volatilization or gas, and inhaled into the lungs. When exhibited in this way, it acts topically upon the part most materially diseased, as well as generally upon the system. At some future period, I shall do myself the pleasure of presenting to you, an essay upon the above mode of exhibiting mercury. I am well convinced many advantages will attend the exhibiting of mercury in a state of gas. Ptyalism may, I am convinced, be produced quicker, more certainly, and without that great tendency which mercury has to run off by stool. In the yellow fever, dysenteries, &c. we are well assured, that mercury, when it excites a ptyalism, is attended with the most sovereign and benign effects. I would therefore advise a mercurial atmosphere for such patients. We are told by Dr Hill, of a ptyalism being produced in three hours by inhaling the mercurial vapour, which arose from some mercurius vitæ, which was suffered to stand upon

\* Quere? the name here meant. It is much to be regretted that proper names are often written so unintelligibly, that it is impossible to decypher them, by any connexion with the accompanying matter.—E.

a heated stove, with a view of purifying it. We have also the authority of Chaptal, of a pyralism being excited, merely by inhaling oxygen gas, which was obtained from the red precip. (oxide of mercury.)—I hope my dear sir, you will excuse my lengthy and imperfect letter, and should be happy to hear your opinion upon this mode of exhibiting mercury. Since cuticular absorption is denied us, how shall we explain the *modus operandi* of the mercurial ointments? Is it not by the heat of the body volatilizing the mercury, thereby creating a mercurial atmosphere, &c. Believe me to be

Dear Sir, your's respectfully,

P. W. LITTLE.

DR. J. R. COXE.

*Analysis of an Iron Ore, containing Titanium; by JAMES WOODHOUSE, M. D. Professor of Chemistry in the University of Pennsylvania.*

*Philadelphia, August 27th, 1807.*

THE following experiments were performed upon an ore found in New Jersey, which I received from Dr. Mitchell of New York, in the year 1805, which was then supposed to be an oxide of zinc, and which Count Bournon, one of the first mineralogists in Europe, has declared to be composed of iron and titanium.

The specific gravity of this mineral is 5.28. When viewed, it has the appearance of black specks, the size of duck shot, surrounded by a red substance; and streaks of a white powder are dispersed through it, which consist of lithomarge or argillaceous earth. Looking at a specimen of it through a microscope, a small crystal of titanium, was seen adhering to it.

One hundred grains of it, reduced to an impalpable powder, and exposed one hour to the intense heat of a powerful air furnace, lost fifteen grains in weight, and from a brown, was



turned of a black colour. Our hundred grains submitted to heat, in the same manner, with charcoal, produced a great number of small globules of pure iron.

This metal can be separated from the powder, by a magnet.

One hundred grains, boiled in aqua regia, was totally soluble in this agent, which proves it contains no silicious earth.

The prussiate of potash, added to this solution, yielded a blue precipitate of prussiate of iron, which, when dried, weighed three hundred grains. Now, if we divide this sum by six, it will give the quantity of metallic iron, contained in one hundred grains of the ore, which is fifty. A portion of lime was thrown down, from a solution of the mineral in aqua regia, by the oxalate of potash. The carbonates of ammonia and potash, produced a copious, white and gelatinous precipitate.

One hundred grains, mixed with six hundred of potash, were submitted to intense heat, in a black lead crucible, one hour. The part remaining in the crucible was powdered, boiled in water and filtered. Upon adding a small portion of muriatic acid to the water, a white precipitate was thrown down, which was supposed to be the titanium.

Upon collecting it, and mixing it with charcoal and spermaceti oil, it was exposed to the heat of a blacksmith's forge, when nothing was obtained, but a portion of a shining, black heavy substance, of the appearance of glass. When the muriatic acid was added in excess, to the filtered water, obtained by boiling the residuum which remained in the crucible, in water, no precipitate was produced, until a solution of potash was added, to neutralize the acid.

The solution of the mineral in nitric acid, is very astringent to the taste.

The ore appears to consist of iron, titanium, alumine, lime, and no silica.

*Extract of a Letter from Professor SILLIMAN of Yale College, Connecticut, to MR. ROBERT HARE jun. of Philadelphia, on the Nitrous Oxide, Communicated to the Editor by Professor WOODHOUSE.*

THE following letter on this subject, from Professor Silliman, of Yale College, Connecticut, has been put into my hands, by Mr. Robert Hare jun. of this city.

"Since my return from Europe, I have given the nitrous oxide a full and fair trial, and the result has been such, as to confirm in the most satisfactory manner, Mr. Davy's account of the effects of this wonderful agent. In my own case, after only two inspirations, I felt a momentary loss of distinct thought, ; then sensations of pure and vehement delight, tingling through every fibre of my frame, to the extremities of my toes and fingers. then, after failing in an attempt to express to my friends by articulate words, the pleasure I felt, I demonstrated it by leaping up and down, stamping on the floor, and loud convulsive laughter.

One of our gravest citizens, a man of thirty-eight or forty years of age, was made to caper about like a monkey, with all the extravagant gestures of a tragedian, and the grimaces of a harlequin. Some effect was produced upon all that breathed the gas, and the full effect was manifested in six instances out of eight. One of these took place before many spectators, and was so marked as to banish every doubt. The reason that these experiments have not generally succeeded is, that the gas has not been employed in sufficient quantities. Six or eight quarts breathed into, and out of a silk bag, will always produce the effect."

#### ERRATA.

Page 136, in Dr. Rush's communication, last line, for "*and medicines*" read "*and tonic medicines.*"

Page 179. in Dr. Woodhouse's communication, line 2, for "*dephlogisticated air,*" read "*dephlogisticated nitrous air.*"

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# MEDICAL MUSEUM.

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VOL. IV....No. IV.

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*A few Words more against Cutaneous Absorption.*

By J.C. ROUSSEAU, M. D. *Physician to the Philadelphia Dispensary.*

July 19th 1807.

**L**ONG ago have I promised, and oftentimes since determined, to pursue my inquiries upon Cutaneous Absorption; but convinced by the evidence of the numerous experiments corroborating the new doctrine that I have established,\* I could but smile, and recollect how much time, and how many useless arguments were wasted by one of my preceptors, to demonstrate that “a straight line was the shortest that “might be drawn between two given points.”

Much has been and is daily said in support of this supposed function of the skin: superstition and prejudices, religious advocates of old erroneous doctrines, too often receive the assistance of pride and obstinacy; arguments which have been com-

\* See my Inaugural Dissertation, 1800. Philadelphia.

2. Henry P. Dangerfield, Inaugural Dissertation, 1805.

3. Joseph Klapp, Inaugural Dissertation, 1805.

pletely refuted, are again and again brought forward, backed by some experiments, which when considered attentively, however deserving of notice they may appear at first view, cannot in any way afford the least help to that falling doctrine.

It is indeed amusing to listen sometimes to a number of facts seriously related, and gravely explained, to shoulder up such an erroneous opinion of the old school, as if the faculty was to lose by its fall, any of its privileges to besmear the body of a faithful patient, who seldom dares to ask for an account of the *modus operandi*, and whom it would be easy to satisfy, (if it was necessary) by only substituting the much more elegant and scientific word *sympathy*.

“Pray sir,” was asking me once a thoughtful gentleman, rubbing his snuff-box, after having quartered his gold-headed cane under his arm, “how do you account Sir,” pausing to load his nose, “for this fact—that—sailors having no fresh water on board, quenched their thirst by plunging their bodies in, and wetting their clothes with sea water.”

Have you seen it, Sir, replied I, with an inquisitive look? “I cannot—say so.”

Nor I either—however, as I am not disposed at present to deprive you of the benefit of authorities, I will grant the fact, (which by the by, if it was put in practice, would save a great deal in many respects) if you dispense me with its explanation until you have yourself accounted for the following observations of the same nature, and established, not by the authority of a few, but by general self-experience.

How often you and I, and many others have experienced a great thirst in the night, and not being able to find any water

at hand, have fallen asleep after having either raised up in bed, or walked once or twice through the room and awakened in the morning perfectly free from thirst; can absorption be thanked in such a case?

Children at night often cry for a drink, and their mothers or nurses not being able to satisfy the poor little sufferers, on their growing noisy and troublesome, give them a whipping; they are hushed, go to sleep and never think of a drink the next morning; has the whipping been absorbed?

Who, ever so little observant, has not remarked that a number of affections of the stomach are removed by impressions upon the mind, upon the skin, the membrana pituitaria, the urinary organs, &c.?

Who is not acquainted with the means vulgarly put in practice to stop the hiccup? has not an active flagellation antagonised the power of an emetick thrown into the stomach? has not the same, judiciously been put in practice, to snatch from the hands of death the unfortunate victim of an overdose of opium?\*

\* The case I am going to relate, offering not only remarks in illustration of these facts, but of other opinions advanced in this essay, and being original in its mode of cure, as far as my knowledge of records extends, I think it cannot find a better place.

A son of Mr. McDowell of Philadelphia, in his third year having swallowed about half a pint of spirituous liquor, fell into a state of intoxication, threatening immediate death. When I saw the child his extremities were cold, the pulsation at the wrists entirely gone, and his jaws closed so tight, that it became impossible to administer any remedies by the mouth.

A strong solution of antimoniated tartrate of potash was conveyed to the stomach through the nose, and stimulants applied to the surface of the body, hands, feet, and the membrane of the nose, without any apparent effect.

Seeing but little chance for the life of the child, unless speedy means were resorted to, I desired the crowd in the room to retire, keeping only one assistant, Capt Berry, brother-in law to the child. I then introduced a catheter into the urethra, and irritating by a smart friction the sphincter vesicæ, in a short time the child screamed out, and with proper management recovered.

Might not the same stimulus be used in a number of instances, when the irritability has departed from the external parts of the body? I would not at all

Nothing, until the experiments have been performed as they ought to be. I may perhaps then admit, as I now conjecture, that the skin of a living body soaked for a long time in water, may imbibe a small portion of it; but this is surely not what you pretend to call absorption? Besides we have as many, if not more authorities to deny this fact, as to establish it.

“But, Sir, will you deny that a number of substances applied to the surface of the body, often create a nausea?”

I admit that, although I have a strong right to doubt it.\* But is the nausea in such cases, created by an absorption of the substance applied? If so, is it not more rational to suppose, I may say, to admit, as I have evidently proved by my former experiments, that such an absorption is performed by the organs of respiration? But it is so common a thing to see an action upon the skin, or upon any of our senses, produce nausea, vomiting, and other derangement of the functions of the animal economy, that we cannot, without injustice, blame it upon the pretended absorbent power of the skin.

The simple smell of a number of substances produces nausea, and even vomiting.

\* I say that I have a strong right to doubt, that the substances said to produce a nausea when applied externally, have that effect; for I have never succeeded in my attempts. I have once applied as much as a drachm of emetic tartar upon the neck of a person, and kept it confined there for twenty-four hours, without producing the least symptom of nausea.

This very day, before breakfast, I have applied and secured by a bandage, thirty grains of emetic tartar moistened with water, upon the pit of my stomach, and forgot it until one o'clock P. M. Having perspired freely on account of the heat of the day, I then found that it had entirely melted away, but never experienced the least effect from it.

Tickling often produces the same effect.

The sight of a disgusting object, even without smell, has created sickness at the stomach: it is a sensation so generally experienced, that it cannot be denied.\*

A discordant music affects singularly the stomach; this is so generally experienced, that among the French who claim the ascendancy of taste in judging of harmony, it has become a proverb. *Cette musique fait mal au coeur.*†

The sight of the human blood brings on nausea, vomiting and fainting.

The relation of a dirty story, of a disgusting anecdote, has in my presence, in company with twenty persons, repeatedly produced vomiting.

The motion of a carriage riding backward, is a sensation that seldom fails to create sickness at the stomach in a great number of individuals; I have experienced it myself frequently.

Sailing, whirling round, swinging, are attended with the same derangement.

\* I once accompanied a gentleman to the museum of the Pennsylvania Hospital. After having examined attentively the elegant preparation in wax, of the internal parts of the human body, he whispered to me to come out, and declared that he was seized with such a sickness at his stomach that he was afraid of losing the benefit of his dinner.

† *Mal au coeur*, by the French, means that sickness at the stomach that precedes vomiting.

A kick on the shin bone, a fall on the coccx, a severe pinch of the toes or fingers, in short, almost any sudden acute pain, produce sickness at the stomach.

Most assuredly none of us will pretend to hint, that either tickling, the sight of an object, sound, motion, a kick or a fall can be absorbed ?

I have witnessed fear produce vomiting and purging with extreme violence, and in one case, sudden death ; I have seen it, not long ago, occasion the emission of the feminal fluid instantaneously, and that without the appearance of any erection.

Indeed, would it not be, shall I be permitted to add, more consonant with solid judgment, to attribute a great many of the effects supposed to be produced by our remedies, to the operation of the mind : for do you want to increase the effect of a remedy, you will surely operate it by informing your patient of what he is to expect from it.\*

“ Now, Sir, the effect of cantharides, when applied in blisters, I hope,” ————— proves as much as all the rest ; for hardly once in twenty cases, does a stranguary take place. I have applied blisters upon the pubes and perinæum,

\* I have known lately of a person having had seven evacuations, from the taking of two pills of *mica panis*. Another had the same number from six drops of *Aq. color. cum tinct. coccin. llæ*, and three more the next day from three drops of the same preparation. If, as it is a fact, imagination can operate the evacuation of the tears, urine and sweat, the liquor of the prostate gland and semen, I see no reason to deny it the power of determining vomiting and the alvine flux.



without producing any appearance of a strangury; nay, I have gone so far as to give

pulv. cinchonæ gr. x.

canthar. gr. vj. M.

three times a day, and six days elapsed before I could perceive any symptoms of strangury.\*

When blisters occasion a strangury, it must be owing to some particular cause, still remaining in the dark, for if the strangury was owing to the absorption of some particles of the cantharides, the effect would, without doubt, be more general. Indeed the disorganization that is produced by the blister on the skin, renders it very unfit for performing a vital function. And moreover the same result happens sometimes after burns and scalds on the skin.†

I do more justice to the excellence of the vital functions of organized bodies; for I do firmly believe, because experience proves it daily, in a thousand instances, that nothing can be absorbed and carried into the general circulation, even when received in the intestinal canal, unless it has previously undergone a complete change, by a perfect digestion.

\* A gentleman under my care took the tincture of cantharides for several weeks without perceiving any symptoms from it.

‡

† I had, several years ago, one of my hands miserably burnt with gunpowder, and it produced a strangury. The great consent that exists between the skin and the urinary organs, does not require the supposition of an absorption, to explain the phenomena that are exhibited by their alternating with each other. When the bladder is very full, an horripilation is felt before evacuating the contents; cold applied to the skin increases the quantity of urine; sweat lessens it. Most children exhibit a kind of chill when they want to make water. I have known ladies to have their children clean, extremely early, by attending to this phenomenon. My children have all been put in breeches between fifteen and eighteen months, and kept clean by a strict attention to that.

It is certainly erroneous to say, that the matter of abscesses is absorbed. Their matter is not absorbed, but a digestive process elaborates it into a *certum quid*, which, being absorbed, undergoes the process of assimilation, and becomes organised and fit to circulate with the other vital fluids, in the like manner as the food is, by digestion in the stomach, prepared to yield the particles necessary to be carried in, and repair the losses of the general mass. I have opened buboes after the digestive process and absorption had been going on for weeks, and have found that the contents were entirely different from the matter that comes out of those which either break out, or are opened before the digestive process has begun.

Almost all the parts of the animal economy, possess some power of securing or getting rid of whatever is foreign and may become hurtful to themselves. Indeed we find through all nature, that living bodies have received from it a wonderful power of resisting destruction, although many agents are often combined against them to operate their dissolution. A complete transformation, a creation, I may say, has often taken place, to serve the sublime purpose of self-preservation.

“But the narcotic effects of opium, my dear sir, when applied externally, appear to me an objection, so strong against your doctrine, that I am afraid you will not easily get rid of it.”

I have never experienced any effects from its application upon myself, and I also declare that I have never witnessed it upon others; although I have used large quantities of opium in poultices, the only narcotic effect that I could perceive, was an abatement of the pain in the part upon which the poultice was applied.

But I cheerfully grant your objection in all the extent you may please to use it; and still more, as I never pretended to make any thing by the new doctrine that I have established several years ago. I will not make use of it to refute your objections, for, I am confident, that, if you had given to the new mode of experimenting that I have embraced, all the force that it justly deserves, you would not now lay such a stress upon these fallacious objections.

I come to your objection.—It includes two suppositions which you at once take for granted.

1st. That opium taken internally is absorbed and carried into the circulation, consequently produces sleep.

2d. That opium applied externally producing an effect nearly similar, it must proceed from the same cause.

This reasoning might do very well for some, who, to save the trouble of investigating, idle in a perpetual state of minority, implicitly believe whatever has been transmitted to us to be sacred and inviolable.

But does the sanction of antiquity preclude the right of revision, and, *S'en suit-il qu'une chose soit vraie parcequ'il y a des siècles qu'on y croit et qu'on la repette?*

To believe that similar effects proceed from the same cause is undoubtedly a very great mistake, for we frequently observe not only similar effects from causes materially different, but different effects from the same cause.

Rocking puts any person to sleep.

A warm room, coming from a cold air, has the same effect.

Combing, stirring gently the hair, scratching softly the head, has a very narcotic power.

The murmur of waters, the shaking of the leaves of trees, a buzzing sound or darkness, lulls any one to sleep.

Have you not often seen a part of an auditory funk into a snore by the monotony of an uninteresting sermon?

I might go further ; but who will ever pretend to say that either rocking, combing and stirring the hair, the murmur of water, or any other sound, may be absorbed.

Nay, I am not afraid to say, that I do not believe the deleterious effects of opium taken internally, to be the result of an absorption ; for, as I have already said, before any substance is absorbed, it must be prepared by a digestion ; and this requires a longer time than we generally find elapsed between the taking and the effect of this substance. It is more probable that the coats of the stomach receive, and by a natural intimate connection with the medullary organ, immediately transmit to it the deadly blow.

I shall not attempt to explain how the head and stomach can react upon one another ; it is one of those mysteries of nature, that will, I am sorry to think, perhaps remain such for ever ; experience proves it daily to those who are in the habit of observing the phenomena of life.

I am induced to favour that opinion, from the result of an operation that I have often had occasion to perform. When an injection is directed into the uterus, an instantaneous sickness is

felt at the stomach, faintness follows shortly after, and in some cases, fainting comes on for a short time. The interval of time between this effect and the introduction of the injection, is so short, that no absorption can be suspected. Besides, absorption being one of the vital organic functions, intended for the purpose of restoration, it is difficult and unwise to suppose, that it may serve also the purpose of destruction. The impunity of swallowing the variolous and venereal virus, nay the venom of the viper, are extremely, if not sufficiently strong facts, to enable me to support this opinion, and establish the improbability of contagion being admitted into the general circulation, after having been carried, by means of the saliva, into the stomach.

So much has been said, and such a stress is still laid upon the effect of mercurial frictions, that I cannot dismiss the present subject, without taking notice of, and exposing the fallacy of the argument deduced from it.

The supporters of the doctrine of cutaneous absorption, no less prejudiced than those in favour of the importation of diseases, never have, and never will, I am inclined to think, favour us with peculiar experiments to strengthen the dying breath of their enervated system. *Analogy*, ever-lasting source of errors, is their sole defence; like a straw in the hand of a child, they use and break their weapon, to strike out a smile from their antagonists.

Let us repeat their so often repeated argument :

Mercury taken internally acts upon the salivary glands ;  
Mercury applied externally produces the same effect ;  
Then mercury is absorbed.

It might as well be contended in this way :

Mercury taken internally acts upon the bowels ;

Mercury applied externally does not produce the same effect ;

Then mercury is not absorbed.

This last argument is certainly intitled to as much credit as the first, and I see no reason why they should not antagonise each other ; or, which is far preferable, be both laughed at as mere sophisms.

The error of attributing similar effects to the same cause, I have already pointed out so repeatedly, that I think it superfluous to expatiate any more on the same subject. But further, that mercury applied externally acts upon the salivary glands, I cannot grant, without this restriction, that it must be applied in such a manner as to favour the free access of its emanations to the organs of respiration ;\* and without this proviso, it never can affect those glands, nor any other part of the constitution ; this is now established by so many experiments, that no shadow of doubt can be raised against it.

Certain it is, that no mercurial preparations, except those in which this metal is preserved in its natural state, but only divided by mixture and trituration, are able to produce a salivation by external application. The *ung. citrinum* has, I know, sometimes affected the mouth, but this has been owing to a cause, that, as far as I know, has never been accounted for. Having observed it myself in a few instances, especially in one, where four persons of the same family, using the same ointment, felt

\* See my Inaugural Dissertation on absorption. See Doctor Daingerfield's and Doctor Klapp's on the same subject.

very sensibly the effect of it upon their mouths, I instituted some experiments to find out the cause. It happens sometimes that by a long friction on the skin, the mercury is revived and the ointment turns black, but unless this change takes place the mouth is never affected by it.\*

This is certainly another convincing proof of the non-absorption of the mercury by the skin, for if we admit it to take place in one instance, we must admit in all, especially when we consider, that by its rancidity the unguentum mercuriale becomes undoubtedly less fit for absorption, than the other bland ointments prepared with the mercurial calces or salts, which never affect the mouth.

The volatility of mercury has been too much overlooked ; and this has been the source of all the errors, with which theories have been overstrained. After seeing patients in the same ward with those using mercurial frictions, sensibly affected by it, we can not refuse volatility to this metal ; and if it is volatile enough to be conveyed from one place to another, through the aerial fluid, can we fairly insist in refusing it admission to, and through the respiratory organs ?†

\* From this observation, I have always cautioned those using this ointment against rubbing it too long on the parts to which it is applied. There must have been, I acknowledge, at the same time, a defect in the ointment, for I never was able to produce this change with the ointment that I have prepared myself.

When essential oils are mixed with this ointment, they turn it black, and render it capable of affecting the mouth, as I have ascertained by several experiments ; but in such a case the ointment is fairly decomposed, and a portion of the mercury revived, for if rubbed upon gold or copper it will amalgamate it.

† My son and another young man had both their mouths very sensibly affected, after triturating, for about eight or ten hours, in the month of July, some old rancid mercurial ointment.

I have never prescribed mercurial frictions. I direct my patient to spread the mercurial ointment upon a leather belt, and wear it upon the abdomen, just below the pit of the stomach.

Now, for argument sake, forgetting for a moment that we have refuted the old doctrine and established the new one, by indisputable facts, can we rationally draw this consequence, that mercury is absorbed by the skin, because the application of a single one of its preparations, increases a particular secretion? We might as well admit that a feather titillating the membrane of the nose is absorbed, because it increases the secretions of the saliva, the mucus of the nose, the lubricating fluid of the eyes, producing, besides the flowing of all these secretions, sneezing and coughing.

But do we not see other stimuli used upon the skin, increase others of the natural secretions?

Frictions of the hand upon the nates produces the secretion and emission of the feminal fluids. Flagellation possesses that power in a higher degree; (*see Meibomius de usu flagrorum in re venerâ.*) The sight of lascivious pictures, the reading of books of the same description, &c. &c. have the same power. Cold applied to the skin, as I have already said, increases the secretion of the urine. Such stimuli, I expect, will never have a claim on absorption.

Was I not apprehensive of trespassing upon the indulgence of the reader, I could relate hundreds of experiments to corroborate those already recited. But as I cannot flatter myself to have conveyed persuasion to every mind, all that I ask from those still retaining doubts, is to resort to investigation, and when convinced, to resolve the *laus tibi sit*, to him who has deserved it.



☞ THE following very appropriate and interesting Lecture was delivered by DR. RUSH, at the request of the President of the Philadelphia Agricultural Society, in compliance with motives impressed by the recommendations of that Society in their publication on the subject of it. He has permitted it to be printed among their Memoirs at the request of the members of that Society, who attended its delivery. That it may be promulgated among those of the Medical Profession, for whose attention it was principally designed, it is also published by his permission in this collection.

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*An Introductory Lecture to a Course of Lectures, upon the Institutes and Practice of Medicine, delivered in the University of Pennsylvania, on the 2nd of November, 1807; upon the duty and advantages of studying the Diseases of Domestic Animals, and the Remedies proper to remove them. By BENJAMIN RUSH, M. D.*

GENTLEMEN,

THE science of medicine is related to every thing. A mere physician, that is, a physician who knows nothing but the sciences which are supposed to belong exclusively to his profession, is a non-entity. To deserve that title in its extensive import, it is necessary for us to know something of the principles and practice of every art, and pursuit of man. There is scarcely one of them that does not furnish some useful facts, or striking analogies, which may be applied to practical purposes, or to the support of some important principle in medicine. Even the science of morals is capable of affording aid to the healing art by its influence upon the understanding through the medium of the passions. It produces this effect in proportion to the extent of the objects to which we direct

our benevolence. The physician who loves the whole human race, will always be actuated with more zeal to extend the usefulness of his profession, than the physician whose affections are confined to the limited circle of his habitual patients. His zeal will be more active, and more impressible upon his understanding, should he descend in the overflowings of his benevolence from the human species, and embrace in his studies and labors the means of lessening the miseries of domestic animals. This part of the brute creation have large demands upon us. The design of this lecture is simply to point out the duty and advantages of studying their diseases, and the remedies that are proper to remove them. The subject is an interesting one to private Gentlemen as well as to physicians, and I entertain too high an opinion of the good sense and correct views of medical science of my present audience to believe, that a few remarks upon it will be deemed an improper introduction to a course of lectures upon the institutes and practice of medicine.

We are bound in the first place to discharge the important duty to domestic animals which I have mentioned, by the relation that has been established between them and us by the author of nature. They were created at the same time, and from a portion of the same dust of which our great ancestor was formed. They are the only part of the brute creation over which man has retained his dominion since his banishment from paradise. We are to them (says Dr. Hartley) the vicegerents of God; and empowered to receive homage from them; and we are obliged by the same tenure to be their guardians and benefactors.\* Their subjection to death, and all the diseases and pains which they feel in common with us, are the effects of the same rebellion against the Governor

\* Observations on the frame, duties and expectations of man. Vol. 1. p. 415

of the universe which subjected Adam and all his posterity to the same evils.

The diseases of the animals which still roam the forests, and refuse to be subject to man, are few in number, and generally of so mild a nature as to yield to the operations of nature. But this is far from being the case with domestic animals. Like the human race, they acquire new and violent diseases by civilization, or by the manner of life to which their connection with us, and their subserviency to our interests and pleasures expose them. Even parturition so perfectly the work of nature in beasts of prey, is often attended with the same difficulty and danger in domestic animals that take place in women. Of this Dr. Bland has mentioned some remarkable instances in his observations upon human and comparative parturition. Similar instances have been communicated to me by Dr. Dewees, as having occurred under his notice while he practised Midwifery in the neighbourhood of Philadelphia.

2nd. We are bound to study the diseases of domestic animals, and the remedies that are proper to cure them, by a principle of gratitude. They live only for our benefit. They cost us nothing in wages or clothing. They require, in exchange for their labor, and all the other advantages we derive from them, nothing from us but food and shelter, and these of the cheapest and coarsest kind, so that there is constantly due to them, an immense balance of debt from us. This motive to take care of their health and lives will appear more striking when we consider the specific benefits we receive from each of them. The horse is not only an important appendage, but a necessary part of the cement of civilized society.—He ploughs our fields,—he drags home our harvests and fruits to our barns and cellars. He conveys them from distant coun-

tries, over rough and difficult roads, to our market towns and sea ports. He receives in exchange from them, the products of foreign nations, and conveys them to the interior and remote parts of our country. He keeps up the inland connection between different states by means of stages and posts, and thus favours the quick communication of intelligence, and the increase of national intercourse, commerce and happiness.—He administers to our health and to our pleasures under the saddle, and in harness. He keeps up society and friendship in neighbourhoods too scattered in its population to admit of visits upon foot. In vain would country churches and courts be opened, without the strength of this noble animal; nor could the great system of representative government be supported in an agricultural country unless he conveyed the elector to the place of suffrage. In maintaining the freedom and independence of nations, the horse bears a distinguished part. When caparisoned with the furniture of war, he feels with his rider, the courage and the pride of arms. In the race, he delights us by his swiftness, in which he exceeds all other four footed animals. Nor let us forget his sagacity in discovering roads, and chusing the safest parts of them, when inattention or darkness, has rendered his rider, or driver unable to discover them. In the physician's midnight excursion to visit the sick, how often has his horse conducted him in safety, (and sometimes overcome by sleep) through imperceptible paths, and across deep and rapid currents of water to the door of his patient, and again, back to his own home. Still further, how often has the convivialist who has sat too long over his evening bowl, owed his life or his limbs to the good temper of this faithful animal, who, in spite of a contrary direction of his bridle, has carried him with unbroken bones to the arms of his servants, to be conveyed by them to his bed in order to dose away the remains of his intoxication.

To the horned cattle we are indebted for many of the blessings and comforts of life. The strength, and patience of the ox in the plough and the team, have added to the wealth of the farmer in every age and country. The cow has still greater demands upon our gratitude. Her milk, in its simple state, furnishes subsistence to a great part of mankind. Its products in cream, butter and cheese, form the most agreeable parts of the aliment, and even of the luxuries of our tables. A pustule upon her udder supplies a matter which when introduced into the body defends it for ever from the small-pox, and without substituting in its room, a painful or loathsome vicarious disease. Millions in every part of the globe unite with us in expressions of gratitude to heaven for this important contribution to the happiness of the human race. But our obligations to this benefactor of mankind, and to her whole species, do not cease with their lives. Their flesh affords us the most agreeable aliment after death. Their tallow and the oil which is interposed between their joints, supply the absence of the sun in candles and lamps, whereby labor and study are profitably extended during a part of the night. Their hair affords a necessary ingredient in the plaster of our houses. Their skins protect our feet and legs in the form of shoes and boots from the injuries of the weather. They furnish likewise coverings for our books and pleasure carriages, and saddles for our horses. Their horns supply us with combs, and even their bones are converted when fresh into aliment, and when dry, into a salt of extensive use in medicine and in a variety of the arts.

Sheep occupy the next rank in the list of domestic animals in their claims upon our science. They afford us by their wool a covering from the inclemency of winter during every year of their lives, and by their deaths they supply us with a delicious aliment in the forms of lamb, and mutton.

The hog is said like the miser to do good only when he dies. But this is so far from being true that he is dishonored by the comparison. He fattens upon the offals of our kitchens, and performs the office of a scavenger in cleaning the streets of our cities from putrefying masses of animal and vegetable matters. At his death, he bequeaths us his flesh for food, his hair for brushes, and his fat for medical and culinary purposes.

The immense and profitable disproportion between the labor of the ass and the mule, and the expense of their food, render their health of great importance in those countries where wheel carriages cannot be employed to convey the products of the earth to a public market.

The goat by its contributions of the delicate flesh of its young, and of its medicinal milk to our use, is entitled to a share of medical attention.

The courage and fidelity of the dog in defending our persons and property from the midnight assassin and robber, and the usefulness of the cat in destroying or chasing from our houses the mischievous animals that infest our cellars and closets, entitle each of them to an inquiry into the causes and cure of their diseases.

It remains only to mention the claims of poultry of all kinds, to a physician's care. They adorn our yards and fruit trees with their plumage. They inform us by their crowing, and other noises, of the approach of day. A part of them furnish us with eggs for aliment, with quills for writing, and with feathers for our beds; and all of them, in a greater or less number at a time, generally constitute after death a portion of our banquets, where a display is intended of hospitality or elegance.

In addition to what has been said in favor of domestic animals in their individual capacities, I shall only remark that collectively, they lessen the solitude and silence of a country life. They please us with their gambols when young, and delight us, by their looks and gestures in mature life, every time they receive food or shelter from our hands. They furnish the means of encreasing and perpetuating the fertility of our lands, and finally they gratify us with a sense of our sovereignty over their labor and their lives; and thus furnish us with a small portion of that pleasure which the father of the human race enjoyed, when he received from his Creator the commission of his extensive dominion over all the creatures that live and move upon our globe.

A third reason why we are bound to study the causes and cure of the diseases of domestic animals, is because nature is wholly *passive* in such of them as are violent, or does harm in her efforts to remove them. This is evident in a more especial manner in the epidemics which sometimes prevail among them. The horses, cattle and sheep, of large neighbourhoods, and extensive districts are often swept away by those general diseases where no aid is afforded from medicine.

4th. By studying the diseases of our domestic animals we may rescue them from the hands of quacks, who add to the mischievous and unsuccessful efforts of nature, the evils of absurd, painful, and destructive remedies. Under this head I shall introduce a passage from the words of Mr. Vial, which exhibits those evils in the most expressive and affecting language. Speaking of the veterinary science, he says, "At this moment all appears obscured or bewildered by the ill placed confidence of the owners of cattle upon the blacksmith of the parish, upon illiterate and conceited grooms, stupid and listless shepherds, or upon a set of men



infinitely more dangerous than all the rest. Who arrogating to themselves the style of doctors, ride about from town to town, distributing their nostrums, compounded of the refuse and vapid scraps of druggist's shops to the destruction of thousands, whose varied disorders they treat alike, neither consulting nature, or art, for the cause or effect.

“ Miserable animal ! bereft of speech, thou can’st not complain, when to the disease, with which thou art afflicted, excruciating torments are superadded by the ignorant efforts of such men, who at first sight, and without any investigation to lead them to the source of thy disorder, pronounce a hackneyed common place opinion on thy case, and then proceed, with all expedition to open thy veins, lacerate thy flesh, cauterize thy sinews, and drench thy stomach with drugs adverse in general to the cure they engage to perform.”\*

5th. It is our duty and interest to attend in a more especial manner to the health of those domestic animals which constitute a part of our aliment, in order to prevent our contracting diseases by eating them. Certain vegetables upon which they feed by accident, or from necessity, impart to the milk and flesh of some of them an unwholesome quality. Great labor sometimes has the same effect. A farmer in New Hampshire, who had overworked a fat ox a few years ago in the time of harvest, killed him and sent his flesh to market. Of four and twenty persons who ate of it, fourteen died, and chiefly with diseases of the stomach and bowels. Putrid exhalations produce obstructions and ulcers in the livers of cattle, sheep and hogs which render them unfit for aliment. They are moreover always unhealthy during the season in which they propagate their species ; hence the wisdom of that church which

\* General Observations on the Art of Veterinary Medicine, p. 16, 17.



substitutes fish for flesh during a part of the spring months. Even the heats of summer, in middle climates, lessen the wholesome quality of flesh,—hence the propriety of living chiefly upon vegetables with a small portion of salted meat during the summer and autumnal seasons.

6th. We are further called upon to study the causes, seats, and remedies of the diseases of domestic animals, by the duties which we owe to our country and to humanity. The products of agriculture and commerce are often lessened by a fatal epidemic, brought on by diseases which blast the character of animal provisions; and many poor families have been left to suffer all the evils of penury and famine, by the death of a single horse, upon whose labor, of a cow, upon whose milk, or of a hog upon whose flesh, they had relied exclusively for subsistence, all of whom perhaps perished by diseases that might have been cured.

7th. By extending our knowledge of the causes and cure of the diseases of domestic animals, we may add greatly to the certainty and usefulness of the profession of medicine as far as it relates to the human species. The organization of their bodies, the principles of animal life, and the manner in which the remote and proximate causes of diseases produce their morbid effects, are the same as in the human body, and most of medicines produce in them, and us, nearly a similar operation. Their acute diseases are the same as ours. They are subject to epidemics from an impure atmosphere as well as from contagions. Fevers, catarrhs—hæmorrhages—dysentery—dropsy—scrophula—vertigo—madness—worms, stone, hydrophobia and apoplexy, affect horses, horned cattle, sheep, hogs and dogs. The rheumatism, angina and tetanus affect horses. Cows are subject to diabetes. Cancers have been observed in dogs. Cats suffer and die from a

disease which appears to be a form of bilious fever. Cutaneous eruptions and sores are common to them all. In short, when we except the diseases which are the effects of certain trades and professions, of intemperance, of the operations of the mind, and of a peculiar function in the female body, there is scarcely a form of disease mentioned in our systems of nosology, but what is to be met with in domestic animals.

To encourage us to extend to them the benefits of medicine, let us attend to the light and knowledge which several branches of our science have already derived from them. During those ages in which it was deemed criminal to dissect a human body, the bodies of domestic animals afforded the only sources of instruction in anatomy and physiology, and even since those ages of ignorance and prejudice have passed away, many important discoveries have been derived from the same sources by accident or design.

The discovery of the salivary glands in an ox by Dr. Wharton; of the fallopian tubes in a ewe by Rufus; of the thoracic duct in a horse by Eustachius; of the lacteals in a kid by Erasistratus, and of the pancreas in a turkey, by Dr. Maurice Hoffman, led to the discovery of the same parts in the human body; and it is well known that the circulation of the blood, and the peristaltic motion of the bowels in man, were first suggested by experiments and observations upon those functions in some of the above named animals.

Many useful hints have been taken from the instincts of domestic animals. They generally retire to places of silence and darkness, and discover an unwillingness to move, and to eat, when indisposed, and thereby teach us the advantages of retirement, rest and abstinence in the beginning of acute diseases.

The approach of epidemics is often known by the sickness of certain domestic animals, or by their deserting our habitations.

Many useful remedies for the cure of the diseases of the human body, have been discovered by observing their salutary effects upon domestic animals. The hellebore was introduced into practice as a purge, in consequence of its purging qualities having been observed in the goat. The use of the seton in certain diseases of the human body, was first suggested by its efficacy in the diseases of cattle. The benefits of frictions in glandular diseases, are pointed out by the improvement in the quality of the milk, and the increase of its quantity, which are obtained by currying the cow.

The benefits of fasting in fevers, are strongly urged by the slow putrefaction of the flesh of domestic animals, which are deprived of food for several days before they are killed.

The benefits of wakefulness, and a standing posture in curing madness, are suggested by the practice of some of the farmers in England, who tame the most intractable and vicious horses, by confining them in a pound, and keeping them awake and upon their feet, by pricking them with a sharp nail, for three or four days, whenever they shew a disposition to sleep or to lie down.

The cure of madness in a dog, by means of a profuse hæmorrhage which followed the cutting off his tail, suggests the propriety of copious blood-letting in the hydrophobia. Perhaps a remedy uniformly certain in that awful disease, may be reserved to reward the successful application of industry and humanity, to its cure, in the affectionate centinels of our houses and our lives.

The safety of bloodletting in old people, is deducible from the appearances of inflammation which are discovered in the bodies of old animals that die of acute diseases. The famous race horse Eclipse, so long known and celebrated at New Market in England, died in the 26th year of his age of a colic, after two days sickness. Upon dissecting his body, not only the whole alimentary canal, omentum and mesentery, exhibited marks of violent inflammation, but the stomach, liver, spleen, lungs, blood vessels and glands, all discovered the same, and other effects of the highest degree of morbid excitement.\* Many other instances of the light which the anatomy, physiology, and remedies for the diseases of domestic animals have shed upon medicine, shall be mentioned from this chair in our lectures upon the institutes and practice of physic.

8th. We are bound to study the means of preserving the health of domestic animals, by all those precepts in the Old and New Testament, which recommend kindness to them, and protection from outrage and oppression. A portion of the humane spirit of those precepts has pervaded all countries, and descended in a particular manner to the nations of the east. One of the tales of a philosopher of India, has recorded this fact in a striking manner. A traveller who was permitted to visit the place of torment for wicked men, saw there every part of the body of a man of high rank in flames, except one of his feet. Upon asking the reason why that part of his body alone was exempted from the rage of the fire, he was told, that the only kind action that man had performed during his whole life, was to liberate a lamb which had been entangled by one of its feet, by means of a briar, in crossing a field, and that as a reward for that act, his foot was exempted from punishment.

\* Vial's elements of the Veterinary art, p. 9, 10, 11.

I proceed in the ninth and last place, to mention a reason for making the health of domestic animals the subject of our studies and care, which I should hesitate in delivering, had it not been sanctioned by the name of a man whose discoveries in physiological, metaphysical, and theological science, mark an æra in the achievements of the human mind; I mean the great and good,—I had almost said the inspired Dr. Hartly—And that is, their probable relation to us in a resurrection after death, and an existence in a future state. I shall read a short passage from the Doctor's works upon this subject. After expressing a doubt concerning the redemption of the brute creation, he adds, "However, their fall with Adam, the covenant made with them after the deluge, their serving for sacrifices for the sins of men, and as types and emblems in the prophecies, and their being commanded to praise God, seem to intimate that there is mercy in store for them, more than we may expect, to be revealed in due time."\*

In favor of these remarks of Dr. Hartly, it may be said, that as moral evil and death accompanied each other in the human race, they are probably connected in the brute creation—That they possess nearly all our vices and virtues; that the perfection of the divine government required that their vices should be punished and their virtues rewarded; that reparation should be made to them for their accumulated sufferings in this world; and that the divine bounty discovered in the gift of their pleasures would be rendered abortive, unless they were placed in a situation to make returns for them, in praise and gratitude in a future state of existence.

It is alike foreign to my inclinations, and to the design of this lecture, to enter further into this question. To such of

\* History of Man. Vol. ii. p. 436.

you as wish to see all the arguments that are urged in its favor, from reason and revelation, I beg leave to recommend the perusal of an essay in the works of Dr. Hildrop, a learned and pious clergyman of the church of England, intitled "Free thoughts upon the brute creation." In whatever way the controversy may be decided, I shall only add, that a belief in the opinion suggested by the physician, and defended by the divine, whose names have been mentioned, is calculated in no one instance to do any harm, but on the contrary, much good, by increasing our obligations to treat our domestic subjects with tenderness and care. If the opinion be erroneous, let the justice and mercy of the SUPREME BEING, in his conduct to his brute creation, remain unimpeached. The divine government in this world, may be compared to the dreary prospect of an extensive and highly cultivated country, on a winter's day. The last revolution of our globe, will clothe this prospect with all the beauties of the vernal, and all the products of the autumnal months. It will then appear that the apparent discord in the *being* and *end* of all intelligent and animated creatures, was

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"Harmony not understood;"

And that all their sufferings were a necessary part of "universal good."

But if the claims of domestic animals be so numerous, and the advantages of attending to their health be so great, and above all, if their high destiny hereafter be in the least degree probable, it may be asked, why do we doom them with so much cruelty to a premature death, and afterwards feed upon their flesh? I answer, that by destroying them we prevent their perishing by hunger, for in the present state of cultivation of our earth, there would not be subsistence for them and their offspring for more than a few years, by which means their species would

soon be extinct. By thus multiplying their numbers, we multiply life, sensation, and enjoyment. We moreover prevent the pains of a gradual death from sickness, and the miseries of a helpless old age. To destroy them by the knife, therefore, and to use them as a part of our food, is so far from being cruel, that it is an act of kindness and benevolence to them.

To the proposal for studying the diseases of domestic animals, it may be objected that their want of speech will forever prevent their imparting to us an account of the seats and symptoms of their diseases. This objection, I am aware, will be urged by those physicians who believe that every disease has a specific proximate cause, and requires an appropriate remedy; but students of medicine, who believe that all diseases have *one* proximate cause, will find no difficulty in discovering their existence and force in dumb animals. The full or frequent pulse, the loss of appetite, the dejected head, and the languid and watery eye, are certain marks in all brute animals of one of the most frequent diseases with which they are affected, that is fever. The watery eye, an inability to bark, or barking with a stertorous hoarseness, indicate the approach of madness in the dog. The elevation of the hair on the back of a cat, and its not falling upon its feet when thrown from a moderate height, are the premonitory signs of that disease which has lately been so fatal to that species of animals in Europe and America. The tail of a horse losing its regularity of motion from side to side, indicates that he is indisposed, and the part in which his disease is seated is pointed out, by one of his ears inclining backwards to the side affected. In acute pains, particularly from the colic, he bites his manger. The seat of diseases in the abdomen where the signs are absent, may be known by pressing the hand upon the whole belly of the animal. It will discover marks of pain, when the diseased part is pressed. Diseases

of the head, lungs, kidneys, limbs and skin, are as easily known as the same diseases are in the same parts of the human body.

There are indeed circumstances, which favour our acquiring a more accurate knowledge of the diseases of dumb animals than of those of own species. From the causes formerly mentioned, the number of their diseases is more limited, and their symptoms are more obvious, for they are not multiplied, nor complicated by intemperance in eating or drinking, nor are they under the influence of passions which suspend or alter them, and in some instances, to prevent their evolutions.

The seats of their diseases, moreover, are more perfectly known from the greater facility of dissecting and examining their bodies after death. Again there are circumstances which favour the operation of medicine upon them, of which we are deprived in our fellow creatures. These are, no prejudices against the names or taste of medicine,—a rare rejection of them after they have been received into the stomach, and the absence of all fear and solicitude, about the issue of their diseases.

I have then gentlemen laid before you, a brief detail of the obligations we owe to our domestic animals, and the reciprocal advantages to be derived from extending to them the benefits of the science of medicine. In performing this task, I have endeavoured to become the organ of speech for the dumb, and a suppliant for creatures that are unable to plead for themselves.

Permit me to recommend the subject to your attention in your future studies. From the knowledge you will acquire of



the anatomy of the human body in this University, of the laws which govern its economy, you will easily comprehend the small deviations from both, which take place in the bodies and functions of inferior animals. By acquiring this kind of knowledge, you will add to the resources of medicine as far as it relates to the human body, and by disseminating it gratuitously in your neighbourhood, you will become the benefactors of your country.

For a while your knowledge in this branch of science, must be acquired by reading, observation and experiments; for as yet no societies or schools have been established for cultivating, or teaching it in the United States.

In all other countries, it has accompanied the advanced stages of civilization. In Greece and Rome, the necessity of offering such animals only in sacrifice, as were perfectly sound, added to the motives for taking care of their health. The Arabians cultivated veterinary medicine with nearly the same zeal that they did the medicine of the human body. In France and Germany the health of domestic animals, has for many years been a part of the studies of regular bred physicians. In St. Domingo, a society called the "Philadelphians," was established many years ago, consisting chiefly of physicians, whose principal business was to investigate and cure, what they called epizootic diseases, that is the diseases of domestic animals. They favoured the world with one valuable publication upon them, before the civil war in that Island put an end both to their labours and their name.

A veterinary school has been lately established in London, under the patronage of some of the most respectable noblemen,

private gentlemen, and physicians in the British nation. Already it has diffused a great deal of knowledge through Great Britain, particularly of the diseases of the horse. Of this knowledge, a considerable portion has fallen to the share of the farmers and farriers, much to the advantage of that noble animal !\*

While I lament the want of a veterinary institution in our country, I am happy in an opportunity of mentioning that the diseases of domestic animals have not escaped the notice of the agricultural society of Philadelphia. They have recommended the study of them in strong terms, in their late address to the physicians and citizens of the United States ; and it would be an act of injustice not to acknowledge, that it was in consequence of the excellent remarks contained in the part of the address to which I have alluded, being impressed upon me with peculiar force by the enlightened and patriotic President† of that society, that I was led to select the interesting subject of our lecture for the present occasion.

But in vain will be the efforts of public bodies, and private individuals to disseminate veterinary knowledge in our country without a provision for regular and oral instruction upon it.

From the public spirit of the trustees of our University, and particularly from their disposition to promote every branch of science connected with medicine, there is reason to believe,

\* The Dublin society of arts have lately established a professorship of the veterinary art, and endowed it with a salary of fifty pounds a year, with a dwelling house for the professor, (Dr. Peete,) valued at sixty six pounds sterling a year. Carr's Stranger in Ireland. p. 99.

† Richard Peters, Esqr.

that it is only necessary to lay before them the advantages of a veterinary chair, in order to insure its establishment.

Should the subject of the diseases of domestic animals, be connected with instruction upon the principles of agriculture, and implements of husbandry, so as to constitute what is called in some European Universities, "economics," or a system of rural economy, it would form a still more useful branch of education, not only for physicians, but for private gentlemen. I have lived to see the medical school of Philadelphia emerge from small beginnings, and gradually advance to its present flourishing condition, but I am not yet satisfied with its prosperity and fame, nor shall I be so, until I see the veterinary science taught in our University.

One of the patriots and heroes of the American revolution, who died suddenly a few years ago in his barn yard, said with his last breath to his servant who stood by him, "take care of the creatures." Nearly in the same words which dictated this kind direction, I shall conclude this lecture. TAKE CARE OF THE HEALTH OF DOMESTIC ANIMALS.

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*Observations on some Morbid affections of the Liver.*

By DR. FELIX PASCALIS, of New York.

**M**ORBID anatomy is of an immense practical utility. Should we not find the genus, the classification nor the name of a disease, it surely leads us, and is a better guide than

any of the newly established nologies. In the two following cases, the one of an almost entire destruction of the liver, and the other of its monstrous growth, after having considered all their respectively instructive circumstances, we are induced to admit a vast number of intermediate affections to which that viscus remains exposed.\* They are no doubt proportionate to the number and importance of its functions.

It is not a little astonishing that the physiology of the liver should remain as yet incomplete and obscure; but while we decline entering into any explanatory theory of its functions† let us be permitted to notice only two principal things among them, and which cannot be controverted. The one is the flowing of the bile, which is incessantly secreted in the liver, into the alimentary tube; and the next is the process of *purification* which venous blood undergoes in the liver, before it passes through the lungs to be made new arterial blood. As for the collection or absorption of lymphatic fluids from that voluminous viscus and from the gall bladder, which by many has been supposed to take up the bile, and transmit it to the thoracic duct, to constitute jaundice, we shall actually say nothing, expressing only our dissent from such a doctrine. Having lost throughout the vascular system whatever elements constitute its vitality, or its oxygene, &c. it appears that other con-

\* However different may be the organization of the liver from that of the gall bladder, their intimate connection shews how equally necessary they are to one principal function, and I do not discriminate those viscera, when I speak of the liver only.

† The ingenious theories of Rush, and Dr. J. Williams, have not escaped my notice. They are both well calculated to direct medical and physiological inquiries on a truly untrodden ground. For the latter, Vide, Lond. Med. and Phys. Journal, No. 100.

stituent principles are set loose that they may be easily separated and evolved into bile; indeed if we were to infer from the well known state of stagnated blood in the *reticular* membrane of the skin or echymoses, to wit, from its change of dark red, into a green and yellow colour, we might conclude that as soon as venous blood stagnates, it deposits its yellow serum, &c.

Therefore the large capacity of the liver, the small share of irritability and sensibility which it is endowed with, are necessarily favourable circumstances for the formation of the bile. Now suppose the circulation of the blood from the *vena portarum*, is obstructed through the liver, venous blood shall insensibly alter its colour and effect a jaundice all over the surface of the body, in those minute vessels of the reticular membrane which first participate in the rapidity or danger of circulation. That primordial cause cannot effect sometimes but a dark red suffusion, attended with various sorts of eruptions or efflorescences, as it happens to a great number of those intemperate persons who drink more than they eat, and therefore consume very little bile.

On the other hand, those persons who from a certain morbid state of the stomach derive a canine or voracious appetite, require a greater quantity of bile for their digestion; in them venous blood passes much quicker through the liver, hence they are always of a pale colour and thin habit of body; with these observations all founded upon facts, we can easily account for those temporary and sudden jaundices which take place in consequence of violent causes, passions, fevers, worms, concretions of bile, and also from specific poison introduced into the venous blood through the reti-

cular system. I have it from respectable authority that the bite of certain venomous snakes in Martinico has been known to be attended with considerable jaundice; and the celebrated Dr. Mead, who has professedly treated of the venom of certain reptiles and insects, has mentioned the frequent occurrence of the same symptom. If venous blood is once infected in the reticular system or elsewhere, it may by irritation, or chemical affinities, be decomposed, and deposit its yellow serum, or if it reaches the liver, it may disturb or suspend its function. On the other hand we are authoritatively\* induced to believe that many cutaneous eruptions are symptomatic to a diseased liver; this is further evinced in the cases of drunkards; it must consequently be very probable that the specific poison of the *Herpes* kind could be transported and inoculated into the liver, and cause its destruction, as happened in the little girl whose case I shall hereafter relate.

We may now conclude that the action upon the liver, of the stomach, of digestion and of certain substances or aliments, must prove very great indeed. It is therefore my duty to advert especially to the use of spirituous liquors. They have some chemical laws and peculiar tendencies, (as it is well known the alcohol in them, is naked in a great measure and very abundant,) as that of thickening the albumen and keeping undissolved every kind of animal jelly. Now if we observe that these two principles constitute animal food, we must conclude that if they are too much diluted with alcoholic fluids, they cannot be dissolved with all other digestible mat-

\* I some years ago, read an excellent paper, in one of the English Medical Journals, which supported that opinion with perspicuity and great correctness of observation. I have since mislaid my note and forgot names and dates. Whoever may recollect that eminent authority, will not, I hope, accuse me of plagiarism.

ters, and must be precipitated as excrementitious results; therefore a proportionately smaller quantity of bile is really employed in the digestion of drinkers. Hence the continual redundancy of that animal fluid in similar subjects, and all the concomitant symptoms of red and yellow suffusion on the reticular membrane, and of various sorts of eruption. Their liver and gall bladder are so much infarcted and distended with bile, while the venous blood does not cease to secrete it in its reservoirs, that they seldom escape attacks of hepatitis, or the liver must enlarge proportionately. Whether alcoholic fluids being frequently poured into the stomach and washing constantly the mucous membrane of the intestines, have a peculiar tendency to fix in the liver the albumen and gelatine of the blood, and to increase thereby the bulk of its parenchyma, I certainly shall not determine; but that such are the facts derived from observations among the various liver complaints to which drinkers are exposed, I am happy to find it cannot be contradicted. We may add other authorities. The epicures of old knew how to procure luscious morsels of overgrown livers of geese, by feeding them with alcoholic mixtures. That practice is well known yet in the French country called Alsace. They improve it still by keeping them in warm and dark places.

Heat increases the secretion of bile because it accelerates the decomposition of venous blood, just as it promotes the suppuration of the latter as soon as it is freed from the action of the vascular system.\* To that same law we must ascribe the nume-

\* If that theory is true which supposes that the liver not only separates the bile from the blood, but also a great portion of the chyle which has not yet been wasted when the blood arrives in it, it would give a key to unveil the mysteries of suppuration and granulation of wounds; for laudable pus could be nothing else but good chyle mixed with a small proportion of bile, and the very element which added to a little arterial blood is turned into sound and good flesh, &c.

rous and fatal diseases of the liver in India, especially in Batavia. Chisholm tells us that the hepatitis of West-India Islands is always attended with enlargement of the liver. In the subjoined case however the overgrown liver was not in the least inflamed, but the patient died with a gastritis.

#### CASE FIRST.

##### *Of a Liver uncommonly enlarged.*

Mrs. H. S. aged 37 years, a well formed person and of a sound constitution, the mother of four or five children, had been in the early period of her life, exposed to the example and practice of drinking spiritous liquors. She soon adopted that pernicious habit, until she absolutely injured her health. From time to time she became subject to ephemeral fevers, mostly attended with mental derangement. These took afterwards more serious symptoms, and lasted a certain period of days, during the intermissions of which, the patient was really insane. Many of these attacks took place during the last six years of her life. When recovered, she evinced a sincere and a firm determination to renounce her former habits, which no reason whatever, no anxiety of mind, no domestic troubles could provoke, as she was associated to an honest and indulgent husband. But she seemed to be under the most insuperable influence, as it may be inferred, from the state of her feelings, when she was deprived of a powerful stimulus. She continued thus in that succession of errors, of disease and of vain resolutions, until she was taken with violent symptoms of gastritis, or inflammation of the stomach. They lasted three days without intermission, and baffled every possible remedy. In that short space of time she was copiously bled every day, and she died, apparently exhausted with puking. No appearance of jaundice had ever taken place in this patient. As no satisfactory account could be given of so violent a death, it was



thought proper to inspect the alimentary canal; for liquors clandestinely procured might have contained some noxious substances, or by their bad quality produce poisonous-like effects.

In the presence and with the help of Dr. J. E. R. Birch of this city, a longitudinal incision of the external teguments was made from the sternum between the *Claviculae*, down to the pubes, which dissected laterally, pulled and folded, the sternum was easily separated from the ribs and raised. Now the thoracic with the abdominal cavities were all at once laid open and exposed to view.

At first, nothing but an immense liver was seen. By raising and distending the diaphragm, it had dislodged the lungs from the anterior part of the thorax. Under it the stomach was squeezed backwards, and the whole mass left no space unoccupied in the left hypochondrium; by it, the great arch of the colon was much lowered, squeezed, inflamed, and strictured.

However large this mass was, and paler than natural, it shewed no mark of disease internally or externally, nor was the gall-bladder overgrown in proportion, although it was much distended with black and ropy bile.

The stomach was the next object of our inspection. The whole pouch having been extracted, it appeared contracted, but offered internally the most intense state of inflammation, it was nearly black, and the *rugae* were lined with a thick and purulent mucus. All the intestines, the spleen, and the bladder, were smaller than natural. The pericardium itself was less than one third of that of an adult, offering the singularity of

the phrenic nerve inserted on its surface, not much thicker than a common thread; in fine, the uterus was reduced to the size of a small pear, hard and very white, and its ovaria proportionately diminished, offered a few of the ova, protruding as it were by the effect of pressure. The weight of that enormous liver was judged to be no less than fifteen pounds.

We were now satisfied that the state of pressure in which the stomach was, under the weight and growing size of the liver, had determined the inflammation; and that under the same cause, the functions of all the other viscera being checked, their moles movenda required incessantly, the addition of new stimulants, without which the patient must have felt herself to be under the horrors of agonizing debility.

#### CASE SECOND.

##### *Of a Liver nearly destroyed by Tubercles.*

THE girl E. G. aged 11 years, had all the appearance of a sound constitution, she could even be called a robust child; having been neglected during some time, she had been exposed to the effects of unclean beds and body clothes, and she seemed to have contracted an eruptive disease, which having attracted the attention of a friend, she was put under the use of some remedies.

This itch partially disappeared, but whether renewed or palliated only, she soon appeared to grow weaker and sickly. I

and informed that during many weeks, her case was considered as fever and ague.

To this succeeded a deep yellow suffusion all over the body ; an habitual costiveness , her fæces being like putty, and much harder too. When I first saw her, the fever was remitting, and she experienced by times a great want of food. Had all these last symptoms marked the beginning of her complaint, it had been justly inferred that she had worms ; but all the aforesaid circumstances, and various appropriate remedies which had been resorted to, rendered that supposition very vague, yet, it was again indulged in, but without the least relief. Other physicians thought that bilious concretions had interrupted the flowing of the bile. To this I observed, that children might be subject to any kind of limy concretion, but that the rapidity of their circulation and digestion must always oppose those of the bile. No ground whatever, no indication could open any favourable prospect ; while her symptoms were daily growing more alarming ; when the little unfortunate, who was groaning under excruciating pains, which she traced to the region of the stomach, fell delirious, and shortly after expired.

This certainly was an anomalous case.

The body exhibited no marks of emaciation, but on dissection, the liver was found almost destroyed, not by decay or supuration, but by tubercles, which appeared on the surface like circular contractions, which succeeded to each other, while the parenchyma was wasting or absorbed.

The little mass remaining, about one third of the common size, offered no shape of great or small lobes ; it was darker

than natural. Its contracted pori-biliarii contained black blood, and the gall-bladder affixed to it, was half wrinkled. The stomach was not much altered, but the whole intestinal canal and omentum appeared in the most complete state of inflammation. The whole colon was diseased and considerably strictured; the pulmonary vesicles were filled with dark blood, and not otherwise diseased.

Dr. Matthew Baille, in his morbid anatomy says, "that the common tubercles in the liver, are hardly ever met with in a very young person; that it is likewise more common in men than in women, that it seems to depend upon the habit of drinking."\* The above case is in every point subversive of his doctrine; the life of a man, is not sufficiently long to embrace as many facts, as would be necessary to circumscribe causes and effects in the history of human diseases: whoever writes a book, finds it very convenient thus to form arrangements and classifications, which if not very correct, will one day perplex our judgment, when we are called upon to relieve or remove a complaint.

*New York, Nov. 12, 1807.*

\* Vide, page 214, Lond. edit. 1807.

*Case of Hepatic Affection.* By DR. J. L. STRATTON.

*Burlington, (N. J.) July 14, 1807.*

SIR,

IF the following statement of a fact shall be considered of sufficient importance to merit an insertion in your Museum, it is at your service.

Near the close of December last, a negro man who had formerly resided in the West-Indies, applied to me for medical aid, affected with cough, shortness of breath, and fever. On examination, a protuberance was to be felt directly under the umbilicus, of the size of the head of a full grown fœtus, and to the touch as hard as iron. Apprehending his complaint to be a schirrus in some of the abdominal viscera, after taking away eight ounces of blood, and opening his bowels with calomel and jalap, I gave the nitrous acid, and directed a blister to the part diseased, which, after raising, was to be dressed with mercurial ointment. While persisting in the above course of medicines the symptoms were mitigated, but he could not be prevailed upon to continue them any length of time.

He was constantly affected with a moderate cough and difficult respiration, accompanied with a discharge of limpid phlegm. He never experienced severe pain, but what pain he felt was seated in the unsound part; he always denied having pain in his shoulder, which is frequently a symptom of that disease, and was seldom prevented from pursuing his business, (which was that of a barber) more than one or two days at a time.

On the 10th instant, he was suddenly seized with delirium, and ran into the street naked. On taking away eight ounces of blood

he became rational, until the next morning at five o'clock he underwent a similar paroxysm of insanity, that lasted about the same space of time as the preceding one, and then left him rational.

On the 12th, at five o'clock in the morning, he became speechless, and continued so until 10 o'clock the 13th, and then expired apparently from suffocation.

In the presence of Drs. E. Shippen and Cole, I opened the abdomen; the liver presented an appearance truly astonishing; it was greatly enlarged, and in a complete state of schirrosity, except a very small part of the concave side of the left lobe, and weighed 12<sup>3</sup> pounds. The gall-bladder perfectly natural and contained a quantity of dark coloured viscid bile.

After raising the sternum the lungs appeared but little different from the common state, with the exception of a few small *pellicles* on the right lobe filled with extravasated air.

I was informed by him that it was five years since he became diseased, and that the affection was at that time about the size of an egg.

He was not remarkably costive, and the usual quantity of aperient medicines would operate upon his bowels. His appetite and digestion were generally good, and he was not emaciated.

What might have been accomplished by the powers of medicine at the commencement of the disease, I cannot say; but at the stage of the complaint in which he was when I first saw him, I am acquainted with no remedy that I think could

have restored him to health. I believe he was for the most part temperate, and eat but little animal food.

I am, sir, yours,

JOHN L. STRATTON.

DR. JOHN REDMAN COXE.

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*Case of Flores Zinci taken by mistake for Magnesia, in large quantities without any effect. By DR. M. WENDELL.*

*New-York, August 1807.*

SIR,

OBSERVING in the last number of the Medical Museum, a case of epilepsy, "highly alarming," and threatening immediate destruction, in which the "flores zinci were given with empiric boldness," and apparently to the complete eradication of that distressing disease, I am induced to offer a few remarks on the efficacy of this *supposed* powerful medicine.

From repeated trials of the flor. zinci in epilepsy, and other spasmodic affections, and from its *never* having the exclusive claim of curing either; being generally preceded by venesection, cathartics, &c. or accompanied with some powerful tonic or astringent, I have adopted the opinion of the celebrated Cullen, who says—"The flor. zinci were introduced into practice by the late Dr. Gaubius, as an antispasmodic, or as I consider it as an astringent or tonic. In epilepsy they *never* answered with Dr. G. himself; nor have they that I know of, though given in much larger doses than he seems ever to have employed. In my own practice, (says Dr. C.) I have not found them of remarkable benefit, nor do I find my fellow practitioners giving a more favourable report."

To those not habituated to giving large doses of any powerful medicine, the exhibition of "uncommon doses," as in the case alluded to, must appear rash and unwarrantable. But may not the same opinions be entertained of some medicines, relative to their presumed deliterious properties, which were formerly, of others? This may be applied to the different chemical preparations, as mercury, lead, &c. A few years since, the acet. plumb. was administered to the extent of  $\frac{1}{16}$  or  $\frac{1}{8}$  of a grain with a "trembling hand" and an anxious expectance of a supervening colic, from this "deadly poison," in similar cases, in which at present, it is given from 5 to 8 grains with confidence, success, and safety; and so with many others.

To convince you that my opinion relative to the inefficacy of the flor. zinci is not premature, and in what estimation is held the "empiric boldness" of the author of the above communication, I will furnish you with a case in which the flor. zinci were *accidentally* taken instead of magnesia ust. in much larger doses, and unprecedented in the annals of empiricism itself.

Mrs. B——, aged 35 years, of a delicate habit of body; having occasional attacks of dyspepsia, cardialgia, and costiveness, for which she was in the practice of taking calcined magnesia.

Through inadvertance of the student,  $\mathfrak{z}\text{ij}$ . of flor. zinci were sent, instead of the usual quantity of magnesia. Of this she took a pap-spoonful two or three times a day, agreeable to the urgency of her symptoms.

Visiting her the second day afterwards, she observed that the magnesia was not the same she had been used to, it being



gritty, and mixed differently with her drink. On examination it proved to be the flor. zinci; and on questioning the student, he acknowledged his mistake. A day or two after I called again, anxious to know (for such is the force of first impressions) whether its exhibition had been attended with any alarming symptoms, as must have occurred from the "uncommon doses," in which it was taken, (not less than 30 grains two or three times a-day) on the supposition of its being so powerful a medicine; the only sensible effects they had on Mrs. B——, after strict inquiry, were, a slight constriction of the fauces immediately after swallowing, which soon left her; and about 15 minutes after their introduction into the stomach, she experienced a warm, glowing and rather agreeable sensation of that organ; they also had the effect of keeping her bowels in the same lax state, as when the magnesia was taken. I avoided mentioning the mistake till some time after. She imagined it had improved her health, and given her a better appetite. The two ounces were taken in less than a fortnight.

Possibly you may entertain doubts relative to the quality of those flowers; to remove these, I herewith send half an ounce of the same parcel precisely; these you may analyze, and if you think proper, give some to Professor Woodhouse, for his inspection also.\*

Yours, &c.

M. WENDELL.

DR. JOHN REDMAN COXE.

\* From every external evidence of the specimen sent, there seems to be no reason to doubt the purity of the flores zinci employed—a part was given to Dr. Woodhouse, who has not yet mentioned to me the result of his experiments on them.—*Ed.*

*Observations on the Waters of Schooley's Mountain (N. J.)*

By DR. LEWIS SWEITZER.

Springfield, June 24, 1807.

DEAR SIR,

IF the following experiments are found worthy, you may give them a place in your Museum. They were made on the Springwater, near Hackett's-town, Schooley's Mountain, in September, 1803.

## EXPERIMENT 1.

I took some of the common oak galls, macerated them in water, after which I poured off some of the liquor into a tumbler, added the spring-water gradually, until it turned it of a black colour, which is done almost instantly. Add to this a little sulphuric acid, to take up the iron, and it will become transparent; muriatic, and other acids, have the same effect. These, and a number of other experiments made, shew the presence of iron in the water.

## EXPERIMENT 2.

I took about equal quantities of the mineral, and lime-water, and mixed them in a phial; the lime in the lime-water was soon precipitated; add to this a little sulph. acid, and the lime will be taken up, held in solution, and be quite transparent. By this and other experiments to the same effect, I was led to believe the water to contain fixed air.

## EXPERIMENT 3.

	oz.	dwt.	gr.
I filled a small phial with brook-water,			
weighing	2	14	9
The same phial was filled spring-water,			
weighing	2	14	5½
Difference of spring water, lighter,			3½

The phial weighed, 1.oz. 6.dwt. 21.grs. Brook-water, 1.oz. 7.dwt. 12.grs. and the spring water, 1.oz. 7.dwt. 8½.grs. This experiment ought to have been repeated, to be accurate, but my object then, was merely to shew the spring-water was lighter, which was my opinion before the experiment was made.

#### EXPERIMENT 4.

In a small portion of the water, I poured some sulph. acid, then added the volatile alkali; this was repeated and reversed; but it did not form the cuprum ammoniacum or any thing like it, from which and other experiments, I am of opinion, it contains no copper, as by some supposed, from the taste, smell, &c. I made a great number of other experiments, with a view to discover the true nature, and medical use of this water, but none of them appeared to throw much light on the subject, therefore thought them not worth noting down. The above experiments were made in the presence of Mr. Upjohn, a considerable chemist, living near the spring, and a number of other gentlemen and ladies.

*Observations and experiments, shewing the effects upon the human body, by often repeating on myself and others.*

When drank, it promotes digestion and increases the appetite; it sometimes increases the action of the arterial system, and perspiration; generally, the discharge of urine; it sometimes purges, and proves emetic; it gives tone and vigour, and creates a general glow of heat in the body, after bathing, in those of sound viscera; but when unsound, the contrary takes place, most probably from its great depleting effect upon the kidneys, and inviting too much heat to the surface of the body, which in part makes its escape during bathing, and accounts for the cold sensation after bathing, in those having phthisis pulmonalis and other complaints of the same nature.

From the above experiments, and from the effects, smell, taste, and feel upon the surface of the body, I think it

evident the water contains iron, carbonic acid, with a considerable portion of bitumen, wanting its common portion of marine salt : the iron is most probably held in solution by the carbonic acid, mixed and suspended in the water with the bitumen ; to the latter, carbonic acid, and the great quantity of water generally drank, I think may be attributed the diuretic effect so common to this water. It seems to have a specific effect upon the kidneys, and urinary organs, and in diseases of these parts, will do much good, particularly in calculus, gleet, gonorrhœa ; also in eruptions of the skin, rheumatism, and old ulcers. If business permits, I intend spending some time at the spring this summer, when I shall more particularly examine the water, the result I will let you know if you wish it.

I have made use of a number of our American vegetables, in the country, particularly of the *podophyllum peltatum*, which I have found an excellent worm destroying medicine. These two years past, I have given it to a great number of children, combined with crem. of tart. to purge, and sometimes with calomel after having the vaccine disease ; I made particular inquiry, and in most cases where I had reason to suspect worms, it brought some from them.

With esteem, I am

Yours, &c.

LEWIS SWEITZER.

DR. JOHN REDMAN COXE.

*Case of Lusus Natura*, by DR. THOMAS GRIFFITH.

*Wilmington, (Del.) 7 Month 27, 1807.*

ESTEEMED DOCTOR,

I HAVE thought it my place to communicate to thee, the birth of a very remarkable child, which came under my notice, and if deemed worthy a place in thy invaluable work

it is altogether at thy service. On the 23d, day of the fourth month 1805, I was called upon, to deliver the wife of a certain Caleb Woodard, who resides in the state of Pennsylvania, Chester county, and township of east Marlborough. On making the necessary examination, found her to be in actual labour, with her seventh child. She was of a delicate habit, and thirty-three years old. The os uteri and its neighbouring parts were sufficiently dilated, and the membranes protruding fast, but could not ascertain what part of the child presented; as the formation was such, and the part that offered, was so enormous, I was almost ready to conclude a safe delivery of the child would be insurmountable. As the futures have ever been my guide in head presentations, I with every attention fought for them, but found them not: I then, feeling myself at a loss what to do, resolved ultimately to make it a footling case if I could accomplish it, in preference to the crotchet, which seemed to be indicated. I commenced the operation, by introducing my hand, then broke the membranes and made the feet of the child, while floating in the waters, and delivered it to the hips the first pain, then making the necessary turns, accomplished the delivery in two pains after. The head was situated between the shoulders, without a neck, the breast very prominent, no hair upon the head, but covered with a shining skin, of a dark lived appearance, adhering extremely close to the cranium; the os occipitis completely flat, and a bony role crossed the superior part of the os frontis, at the usual determination of the hairy scalp; under which protuberance were placed the eyes, without the superciliary arches, and of very considerable magnitude; they were red and staring, nose flat and short, mouth not unnatural, the ears sharp and long, presenting forward; part of the upper and lower extremities viz. legs and arms, natural; but hands and feet, bore a resemblance to the monkey tribe. It made a considerable struggle when it visited the world, and died in about ten minutes.

I examined the disconsolate mother at a proper time, if she had been frightened, in the early time of her pregnancy ; she answered in the negative, but said she had been in the Philadelphia Museum, and left it without having her mind impressed with any curiosity which she saw there.

An expression made by Dr. Vaughan of this place, in a case of *lufus naturæ*, very sensibly occurs to my mind ; that man was not only fearfully and wonderfully made in general, but sometimes grievously and wonderfully deformed.

I am with due respect,  
thine to serve

THOMAS GRIFFITH.

DR. JOHN REDMAN COXE.

*Account of a Monster by DR. YATES, communicated in a Letter to  
Professor WOODHOUSE.*

*Albany, June 15, 1807.*

DEAR SIR,

I HAVE it in my power to communicate something new and extraordinary to you.

A very extraordinary animal was produced by a three year old heifer, near the Unadilla river, in the western district of this state. On the 26, of last April, in the afternoon, the heifer exhibited symptoms of great distress, which increased, and urged an almost constant bellowing ; this, together, with the great agony she appeared to be in from her conduct, induced the owner to shut her up in the stable under the impression that "she was getting mad." Her bellowing continued till near day break ; the owner supposing her dead, got of out bed and went with a light to the stable, when to his surprise he found her licking what he supposed was her calf ; on a nearer approach he discovered its form different from what he expected, and attempted to take it up by the legs, when the heifer darted at


him with violence, struck her hoof against the monster's head and broke it; he however took it home, but is not positive whether it shewed signs of life, he thinks it did; he says he did not suspect the heifer to be with calf.

The owner of the heifer sold it to two countrymen, who brought it to this city and immediately procured a five gallon jar, put it in and filled it with spirits. I prevailed on the man to take it out and permit me to examine it for a few moments; he gratified me, and I shall endeavour to give you an outline of this monster.

From the forehead over to the back of the neck, it very much resembles a child; indeed the head from a back view would be mistaken for that of a child. The nose resembles about as much that of a negro as of a calf, I think rather more.

The eyes (or rather sockets without eye-balls) are situated about three inches from each side of the upper part of the nose, in the side of the head, on a line with the os nasi.

It has no distinct upper jaw; a few loose bones are felt immediately on the introduction of the finger into the mouth, under the nostrils.

The under jaw extends about one inch beyond a line with the tip of the nose, and measures in all about two and a-half or three inches, and is shaped like a harpoon, (thus ). It has no tongue. The mouth inside apparently rough, but feels smooth. Ears much like those of a calf, though small in proportion and placed unnaturally back, near, or on the neck; they are about two inches long, and one and a half broad.

The arms or fore legs resemble the human; there is one joint more; it measures from the shoulder to the elbow about four inches; from the elbow to the next joint (which is half way between the elbow and hoofs) three inches; from thence three

inches to the wrist ; in this division are two bones as in the human frame ; from here the hoofs or webbed fingers begin to extend themselves, dividing into five parts, distinguishing thumb and fingers, having at the extremity small marks or spots where the nails should have been, the substance of the hand or hoof the same as of the calf. From below the under jaw or chin down to the pubes, human ; breast broad, abdomen *full*.

Immediately below the pubes, are two appendages like the teats of a cow, (and about as far apart from each other) or, from their flaccidity I might more properly compare each to a separate infant scrotum ; the one is as large again as the other.

No organs of generation—has an anus situated properly ; and a small tail about three or four inches above the anus up the back, about the thickness of a crow's quill, and an inch and a-half or two inches long.

From the haunches down to the knees apparently human, no knee pan, in other respects, down to the hoofs or toes, similar to the arms and hands.

Its form evinces that its natural posture in walking would have been erect ! The skin appears exactly like the human, and the colour a darkish yellow. No hairs, excepting a little on the arms and legs, of a reddish cast and hardly perceptible. At the first glance every one is impressed with its resemblance to a negro child, and many suppose it the issue of a man with the heifer. I hope it will go to your city.

Yours &c,

CHRIS. C. YATES.

Dr. JOHN REDMAN COXE.

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# MEDICAL AND PHILOSOPHICAL REGISTER.

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## FOREIGN AND DOMESTIC.

*Soho Square, August 9, 1806.*

At a numerous meeting of the faculty, held this evening at the house of the Right Hon. Sir Joseph Banks, Bart. K. B. President of the Royal Society, &c. Dr. Harrison laid upon the table the Answers he had received to the different circular letters transmitted to the public bodies and individual practitioners of the united kingdom, in pursuance of a former resolution. He then presented the following Plan for better regulating the practice of Physic in its different branches; which being read and considered, the subsequent Resolutions were entered into.

### P L A N.\*

1st, **T**HAT no person shall practise as physician, unless he be a graduate of some university in the United Kingdom, and has attained the age of twenty-four years. That he shall have studied the different branches of physic in an university, or other respectable school or schools of physic, during the space of five years, two of which shall have been passed in the university where he takes his degree.

\* The necessity of some regulations in the various branches of medicine, being sensibly felt by those who have an opportunity of estimating the unbounded increase of empiricism amongst us; and the subject having been introduced into our legislature during the present session; although thrown out by ignorance, &c. the Editor presumes the following remarks may not be altogether misplaced.

2dly, “ That no person shall practise as surgeon under three and twenty years of age, nor until he has obtained a diploma or license from some one of the royal colleges of surgeons or other surgical corporations of the united kingdom. That he shall have served an apprenticeship of five years to a practitioner in surgery, and afterwards have spent at least two years in the study of anatomy and surgery in a reputable school or schools of physic.

3dly, “ That no person shall practise as an apothecary until he shall have served an apprenticeship of five years to some regular apothecary, or surgeon practising as an apothecary; that he shall have studied the different branches of physic in some reputable school or schools during the space of at least one year, and shall have attained the age of twenty-one years.

4thly, “ That no man shall practise midwifery, unless he has attended anatomical lectures twelve months, and received instructions for the same term from some experienced accoucheur, and shall have assisted at real labours. And that no female shall practise midwifery without a certificate of fitness and qualification from some regular practitioner or practitioners in that branch.

5thly, “ That no person shall follow the business of a retail chemist or druggist, unless he shall have served an apprenticeship of five years to that art.

6thly, “ That none of these restrictions be construed to affect persons at present regularly practising in the different branches of medicine.

7thly, “ Whether physicians shall be entitled to recover their fees by the usual legal means?

8thly, " That a register shall be kept of all medical practitioners in the united kingdom, and every person in future entering upon the practice of any branch of the profession shall pay a fine on admission, the amount and disposition of which to be settled and specified hereafter.

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**RESOLVED,**

1st, That it appears from the returns to the circular letters, that the abuses complained of do exist to a great degree in every part of the united kingdom; and that the necessity for adopting regulations for their correction is universally admitted.

2dly, That it seems to be expedient that the plan proposed by Dr. Harrison be adopted as the basis of regulation; subject, however, to such alterations as may hereafter appear to be necessary.

3dly, That Sir John Banks and Dr. Harrison be requested to wait again upon the right honourable lord Henry Petty, to state to him the progress of the undertaking, and to consult him upon further measures.

4thly, That the following gentlemen be appointed a committee to confer and correspond with the different public bodies of the united kingdom, upon the subject of the proposed regulations; that they be requested to report their proceedings from time to time, and to take such other steps as they may judge necessary. Names of the committee: Sir John M. Hayes, Bart.; Sir Walter Farquhar, Bart.; Doctors Blackburn, Harrison, Garthshore, Pearson, Stanger, Willan, Clutterbuck, and Secretary.

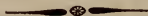
5thly, That a voluntary subscription of one guinea each be received from the town and country practitioners, by any member of the committee, to enable them to prosecute the important objects in which they are engaged. (The names of subscribers to be published hereafter.)

6thly, That Dr. Harrison be requested to circulate the above plan and resolutions of this evening among the Faculty of the united kingdom, in the manner of the former circular letter.

7thly, That since persons of every rank and occupation in life are deeply interested in the proposed regulations, the Faculty are particularly requested to submit them to the principal inhabitants of their respective districts, by convening meetings, or in any other mode which they may think proper.

8thly, That the thanks of this meeting be given to the Right Hon. Sir Joseph Banks for his continued attention to the association, and the important objects of their pursuit.

*Med. and Phys. Jour.*



### *On the Use of Tar-water in Syphilis.*

Dr. *Erich Acharius*, a Swedish physician, has found tar-water of great advantage in inveterate venereal affections, spreading ulcers, pains in the bones, caries and exostoses, &c. given in the quantity of from one to three pints in twenty-four hours. According to the observations he has made in the hospital at Stockholm, out of *seventy* patients treated by the use of tar-water both internally and externally, *twenty-four* were radically cured by this remedy alone. Thirteen others, to whom mercury had

been before exhibited without any effect, were cured by taking the tar-water along with mercury. In seven other patients, who had used in vain the nitric acid and oxygenated ointment, the combined employment of tar-water was found sufficient to accomplish the desired end. Of the patients thus treated, eleven were afterwards subjected to the mercurial treatment, in order to ensure further a complete cure. In eighteen cases, the tar-water having failed alone, recourse was had to mercury. M. *Acharius* concludes, from the whole of these trials, that tar-water merits the greatest attention in the treatment of syphilitic disorders; that it is often sufficient of itself for a cure; and that it is in all cases an excellent auxiliary to mercury, especially in inveterate states of the disease. He employs two pounds of tar to four pints of water.

*Med. and Chir. Rev.\**



*On the Medicinal Properties of the distilled Water of the Lauro-cerasus, or Laurel Water.*

A German physician, Dr. Wurtzer, has observed that the distilled water of the lauro-cerasus (*prunus lauro-cerasus*) diminishes the too great irritability of the heart and muscular fibres, and augments at the same time the action of the absorbent vessels. He has lately employed, with complete success, this water, in the dose of fifty drops, repeated three times a-day, in a case of hypochondriasis. If this character of the laurel water be well founded, it promises to be a valuable remedy, as a palliative at least, in organic affections of the heart, but especially in nervous palpitations of this organ.

*Med. and Chir. Rev.\**

\* From the *Annales de Lit. Med. étrangère.*

*Cure of Incontinence of Urine by Ol. Petrolei.*

Dr. Fichtmeyer has succeeded in perfectly curing a case of incontinence of urine in a woman, by the administration, internal as well as external, of the *oleum petrolei*. This medicine had often before been found serviceable as an external application in cases of paralysis. *Med. and Chir. Rev. from the same.*

*Gelly producible from Ivory Shavings.*

It appears from some experiments of M. *Schroeder*, an apothecary of Berlin, that ivory shavings, boiled for some hours in water, give out a gelly in considerable quantity, and of the most agreeable flavour. By boiling two pounds of the shavings for four hours in three quarts of water, he obtained as strong a gelly as from four pounds of ox-bones. This practice is now adopted in the great hospital at Berlin.

*Med. and Chir. Rev.*

*Hoffman, on Plica Polonica.*

*The statements of the ravages produced in the French army, by the Plica Polonica, have induced the Editor to introduce the subsequent account of a disease, so extraordinary in its nature especially as it is so little known in this country, from the writings of medical men.*

Diseases, the tendency of which is fatal, and the occurrence frequent, peculiarly claim the attention of the practical physician; while morbid affections which appear more rarely, and

present unusual phænomena, more especially attract the inquiries of those whose object is the extension of general science. The disease termed *Plica Polonica* is of the latter class. It is endemic in Poland, and seldom, if ever, observed in any other part of Europe. During a long stay at Breslau, in Silesia, Mr. Hoffman had frequent opportunities of observing this disease; and, as it is at present little known in Britain, he trusts a brief narration of the principal circumstances connected with it will not prove uninteresting.

‘ Both sexes, he observes, are equally liable to the attacks of plica. It usually appears during infancy, and but seldom after the age of twenty. When once produced, it continues during the remainder of life. The accession of the complaint is in general preceded by irregular spasmodic affections, pains in different parts of the body, a slow fever, and various diseases of the eyes; all which cease immediately on the appearance of the plica.

‘ The disorder consists in a preternaturally rapid growth of the hair, with a copious secretion of a viscid matter from its bulbs. For the most part, the hairs of the head are alone affected; and that only in particular parts. In these, the hairs grow considerably longer than in the rest, and are knotted and entangled with each other; being also covered with the viscid matter which issues from their roots, and which assists in gluing them together.

‘ In proportion as the quantity of this gluten, and the implication of the hair increases, it is still more and more difficult to clean and comb it; hence a degree of phthiriasis is produced, and the head contracts an extremely foetid smell, to which, however, the Polish peasants are so much accustomed, that they endure it without complaint, or any manifest inconvenience.

‘ It is also an opinion universally prevalent with them, that the disease is a salutary effort of nature to expel a morbid matter from the body ; and that to interrupt the course of it, would be productive of imminent danger ; hence they make no attempt to cure, or even palliate the complaint. And if we may repose confidence in authors of established reputation, morbid affections of a similar nature to those which precede its occurrence, paralysis, and even death itself, have succeeded imprudent attempts to check the progress of the disease. In this respect, plica bears some analogy to the exanthemata, and various chronic cutaneous eruptions.

‘ I am as yet (he adds) unable to decide whether this complaint is hereditary or not. From some observations, indeed, it appears, that a predisposition to it may be transmitted from the parents to their offspring ; but my information on this head is too limited to ascertain the point. In one case which fell under my own observation, two brothers had plica, both on the left side of the head, and in about one third of their hairs ; I learned from them, that their father and grandfather had also been affected with the disease in a form exactly similar.

‘ Besides the human species, other animals are subject to this complaint. It appears in some of the finest horses in Poland. In them it is situated in the mane, and sometimes in the long hairs around the hoof and fetlock joint. It attacks also the different species of the canine genus ; dogs, wolves, and foxes. Previous to its occurrence in the first, the symptoms of rabies usually appear ; the tail is dropped between the hind legs, there is a flow of frothy saliva from the mouth, the sight and appetite are impaired, or entirely lost ; they are snappish, and disposed to bite, but their bite does not produce hydrophobia. The wolf is affected in the same manner ; he leaves his wonted concealment in the woods, and runs wildly among the flocks,



biting and destroying them, but without producing hydrophobia.

‘The impossibility of ascertaining the true causes of this singular disease, has given rise to several vague conjectures on the subject; as that of Le Fontaine, who attributes it to a corruption of the fat. It is somewhat remarkable that plica takes place only among the lower class of people, whence some have conceived, that it is to be considered merely as a consequence of uncleanness.

‘But, in objection to this opinion, it may be urged, that it is unknown in the adjoining countries subject to the Prussian government, where the peasants are habituated to the same customs and mode of life, or nearly the same as in Poland; that its appearance affords evident relief to the system, and its retrocession is productive of dangerous consequences. The idea that it is a real acrid idiopathic disease, is confirmed also by its occurrence in a variety of animals, and by the circumstance of its being confined to particular parts of the head; for which no reason can be assigned on the former supposition. A peculiarity of climate cannot be adduced as a cause of this disease. Poland differs little in this respect from the adjoining countries. The summer heat is considerable, the thermometer rising frequently to 98°, 100°, 104°; and the cold in winter so great, that it falls sometimes 10, 15 degrees below 0. But though the changes in the atmosphere are so remarkable, at different periods of the year, they take place with the utmost regularity, the temperature passing by insensible degrees from one extreme to the other.

‘The Poles themselves are a vigorous hardy race, inured from infancy to labour, and to exposure to the vicissitudes of

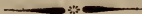
the atmosphere. Almost regardless of cold, they frequently sleep in the open air. Their diet consists chiefly of animal food, and they are much addicted to the use of spirits. They have an equal fondness for other strong stimulating liquids. I have seen them drink with the greatest pleasure the salt brine in which herrings have been preserved, and even nitrous acid diluted with water.

‘Since no other cause can be assigned for this disease, it is probable, that it arises, according to the general opinion, from contagion; a contagion which, like that of Pſora, can be communicated by contact only; but this I have not been able to ascertain by any observations of my own.

‘It is said, however, by authors of reputation, that plica is frequent in Tartary; and that it was brought into Poland in the thirteenth century by the Tartars, who at that period made frequent irruptions into the eastern parts of Europe.’

*Med. and Chir. Rev.*

*from 4th V. of Mem. of the Lit.  
and Philos. Soc. of Manch.*



#### DE LA FONTAINE on *Plica Polonica*.

The first article is entitled, *Surgical and Medical Treatises on various Subjects respecting Poland*. By F. L. DE LA FONTAINE, *Aulic Counsellor and Surgeon to the King, &c.* The only treatise of which an account is here given, is on the Trichoma, or Plica

Polonica, a disease endemic in Poland, and the neighbouring countries, in which a morbid matter is critically deposited upon the hair, and binds it together in such a manner, that to unravel it is impossible. Experience shews, that it is contagious, and very often congenital. There is no certainty when or where it first arose; the Arabians, Greeks and Romans, do not mention it; but some modern writers make the date of its origin, 1387, and add, that it came from Tartary.

The symptoms which precede this affection are various, and such as usually usher in eruptive diseases in general.

‘The proximate cause of this disease is, according to M. de la Fontaine, a peculiar morbid matter, which is clammy and acrid, has its seat in the lymph, and is deposited critically upon the hair or nails. Its exciting causes are equally uncertain, for neither the air, water, nor food, seem to have any effect in producing it; nor are cleanliness and regular combing of the hair any defence against it.

‘In the beginning of the disease, M. de la Fontaine recommends resolvent, attenuant, saponaceous, demulcent, and emollient remedies, to prepare the morbid matter for the crisis. If these be not sufficient, he adds extract of aconite, or cicuta, calomel, or some antimonial. In general, he says, antimony is as specific in this disease, as mercury is in lues. If it be complicated with lues, corrosive sublimate produces the very best effects; but salivation is highly detrimental in every case. In order to bring about the crisis, he makes use of sudorifics. The lycopodium is praised by old physicians as a specific; but unjustly. These remedies can only be used, when no fever is present; in this case, blood-letting and evacuants must be very cautiously used. Our author compares it to the small-pox, where, when

the fever is too weak to produce the eruption, it must be increased; when it is too violent, it must be diminished. Hence the strength of the patients must be often supported with generous diet. The people believe in a number of specifics, but the disease has not become less frequent. External remedies are almost always necessary, such as the application of warmth to the head, in the form of vapour, warm bath, or decoctions of various plants; a decoction of soap is often of great use, when the head-ach is violent. Sinapisms and blisters are likewise applied with advantage. If the morbid matter be deposited on the surface of the body, it occasions malignant and obstinate sores, which give a great deal of trouble. Antimony must always be an ingredient in whatever is applied to these. If the matter have a tendency to deposite itself in the nails, it must be encouraged by the application of stimulants; such as, tincture of cantharides, blisters, or by touching a fresh plica with the fingers. Against the vermin, hair-powder rubbed with mercury is the best remedy. If all these means be inadequate to produce the crisis, inoculation of the disease will often effect it. It is performed by putting on a cap which has just been worn by one who has had a recent plica.

After a complete crisis, the plica separates from the head, and remains attached only by the sound hair. If it has become dry, and all symptoms have ceased, it may be cut off. On the contrary, if recent, and the symptoms still continue, its removal is attended with great hazard, often inducing other violent diseases.

*Med. and Chir. Rev.*  
*from Duncan's An. Vol. 1.*

The following Review of a Work entitled, "*Phænomena und Sympathetie der Natur nebst dem wunderbaren Geheimniss Wunden ohne Berührung vermöge des Vitriols nach K. DIGBY, blos sympathetisch zu heilen*," i. e. The Phænomena and Sympathy of Nature; together with the wonderful Secret of curing Wounds without touching, simply in a sympathetical Manner, by Means of Vitriol, according to K. DIGBY;" will tend to shew that the Days of SIR KENELM DIGBY still exist (or did lately), on the Continent of Europe.

THE author of this work is M. P. Coelstin Störk, a Benedictine monk in Banz. His intention is to take under his protection, what is called sympathy, and all sympathetic cures; and to illustrate them from natural laws. In doing this, he exhibits proofs of much reading; and upon the whole, an exertion of learning, worthy to have been bestowed on a better and more important subject. The ground upon which the author builds his whole superstructure, to prove the existence of sympathetical cures, is, that the thing itself exists, therefore it is possible; and all we have to do, is to find out the causes why it exists. With the proofs of this existence, however, the author, who pretends to leave nothing unproved, goes very superficially to work. He admits as true, without any historical criticism, every account of sympathetical cures and appearances; and endeavours to find out the laws of nature, according to which these appearances follow. All those known accounts of sympathetic appearances and cures, described by our countryman Sir Kenelm Digby, are, with him, proved historical facts: it is in his eyes a true prognostic of the speedy dissolution of a sick person, when dogs howl without any known cause; or when the bird of death, the owl, makes a screaming in the neighbourhood. He even receives as true, the most incredible histories of the effects of imagination of the mother,

upon the child in the womb, without at all examining them. Nay, he confidently tells the fable after Digby, that a man, or even an animal, feels the most excruciating pain in his posteriors, when his excrement is burnt; and that man and beast may be brought into the greatest danger of life, if their excrements are slowly dried in smoke. In a cow, whose milk is boiled away one third on the fire, the udder inflames, and instead of water she voids blood. All those stories of the sympathetic powder, &c. he relates as occurrences whose truth is proved. His theory for the explanation of these appearances is as follows; there is a twofold sympathy, the one in the souls and bodies of men, and the other in the bodies of the universe. This sympathy excites a similarity of affection, and has its origin in the great law of love, which is the bond that unites all beings, and rises to the very divinity from which it flowed. The physical causes of this sympathy lie, in the immense number of atoms and small particles, which are dispersed through the atmosphere, and which often suffer a peculiar attraction; still with the design, that the attraction of homogeneous parts, or of such, which, in both the attracting bodies, are of one figure, one being, and one nature, is much greater; and that, when once such an attraction takes place, every thing which adheres to the attracted body, and which is united with it, is also at once taken away and drawn along with it. By means of these positions the author explains all sympathetical appearances. If for example, when the milk boils upon the fire, and runs over, the cow instantaneously has the most excruciating pains in her udder, provided it is not prevented by a sympathetic effect of throwing a handful of salt in the fire; this is explained in the following manner. The milk that fell on the burning coals becomes changed into vapours, which disperse themselves every where in the air, and are further dragged along by the air and the beams of the sun, and arrive, in conjunction with the

atoms of the fire, to the udder of the cow which gave the milk. The udder has an exclusive susceptibility of attraction for the vapour, because it was the source from whence the milk flowed. The udder being now very tender, and, on account of the teats, being much subject to inflammation, the conclusion of course arises of itself, that through these atoms, the udder becomes inflamed, swells, becomes hard, and knotty. The reason why the salt thrown into the fire prevents this, is as follows: the salt is cool, and of a more fixed nature, and precipitates the fire; for which very reason, a chimney on fire becomes extinguished when salt is thrown on it. Upon suppositions similar to those adduced, nearly all the explanations of the author are grounded. The effects of the famous sympathetic powder, he explains in the following manner: The atoms of the vitriol, in the lye of which, the cloth, stained with the blood of the wound, is laid, are possessed of cooling and healing powers, and force themselves into the wound, in the same manner, as the atoms of fire, milk, and salt, into the udder of the cow, and effect a cooling and healing therein. The existence of maternal marks, the author conceives he has sufficiently explained. He takes the well-known fact for his foundation; that when two stringed instruments are tuned to the same pitch, and the string of one is touched, the corresponding string of the other sounds without being struck. Now the mother and the child in her womb are in a much closer connexion, than these two instruments: consequently, the touching of a conceiving string of the mother, must have an effect upon the consonant string of the child, and must produce in its imagination, (in the imagination of a child in the womb!!) as well as in its body, the same effects. A mulberry fell upon the bare neck of a woman with child; she was delivered of an infant with a mulberry upon the very spot. The imagination of the mother was wholly filled by the particles of the mulberry: she became greatly



moved and astonished. A good part of these atoms, or spirit of the mulberry, went towards the brain of the child, and at the same time to the very spot of the body, in which the mother had received the impressi<sup>o</sup>n: and the spirits, accompanied by the atoms of the mulberry, made in the skin a deep engraved mark; as kindled gunpowder on the face!

*Med. and Chir. Rev.*



*Review of JONES' Medical, Philosophical, and vulgar Errors, of various Kinds, &c. &c.*

A MORE fertile field of both amusement and instruction, can perhaps hardly be found, than the one our author has chosen to employ those hours of leisure, which sickness and confinement had occasioned. He thought justly, that he could not be better engaged, than in breaking a spear against such deep-rooted medical errors, as have been permitted to travel down to us from time out of mind, as matters of unquestionable veracity. It is no inconsiderable step, he observes, in favour of science, to bring ourselves to doubt of the reality of some facts advanced for truth, not only by the ancients, as from their own knowledge, but by some moderns also, in spite of the celebrity of their names. From the various remarks of the author, we shall, for the entertainment of our readers, if not for their instruction, give a few of the most striking.

‘These instances, as I had naturally a narrow swallow, and being no Roman catholic, have not been in the habit of crediting marvels, have given me an utter disrelish for marvellous cures performed, even by men of eminence, though attested also by men in high stations; insomuch that I find great diffi-



culty in giving implicit credit to Sir Kenelm Digby's wonderful success from his sympathetic powder, though attested by his friend the great Lord Herbert of Cherbury ; which, by being only simply rubbed on the sword, which gave the wound, cured the dueller whose body it had run through, without the painful probings and dressings of a surgeon, or the least confinement. I have also a great backwardness in giving credit to Greatorex's cures, and the French impostures of animal magnetism. As for that beautiful experiment, in proof I suppose of his doctrine, that blood has life in itself, which John Hunter contrived, equal to any of Taliacotius's, of transferring a cock's spur from his leg to his head, and making it grow there ; I am sure I have no reason to object to it, as I won a wager that it was not true, which a gentleman paid me freely, upon its having been omitted in a subsequent edition of the quarto Treatise on the Teeth. Though the transplantation of a cock's spur, when successful, appears not to be attended with sufficient advantage to encourage the practice ; that most ingenious experiment, recorded in the Berlin Medical Memoirs for the year 1778, of hatching chickens in forty-eight hours by electricity, not only saves the poor hen nineteen days painful attention, but might turn out a most beneficial article in trade, and reduce the price of poultry, which is at present enormous, if it succeeded here.

‘ Notwithstanding the above strictures, as I myself have a propensity to a little whimsicality, I am ill-disposed to sneer at any medical *jeux d'esprit* ; though they may happen not always strictly to answer the ardent wishes of their projectors ; from a conviction that the laugh of the unlearned at a first project may at times have proved detrimental to science, by so cramping a man of genius in his ardour for improvements, as to intimidate him from making a second attempt, who with a little encouragement might have succeeded better in subsequent trials.

‘ Under this predicament, as simple a scheme as any, was that of the celebrated Leuenhoek, planned for the preservation of his own health, who had such remarkably good eyes, and was so much accustomed to make microscopical observations, that he discovered the small vessels or pores through which insensible perspiration is carried on, which open sideways under the *cuticle*, of such minuteness that 100,000 of them might be covered, he assures us, by a single grain of sand; yet his apparatus did not cost him a penny, being a single microscope made by holding a small glass wire in the flame of a candle, till it melted into a little round globe, which he afterwards placed in a hole made to fit it by a pin in a thin bit of stick. With this he was enabled to make greater discoveries than any subsequent philosophers with their very costly microscopes, which though they magnified, darkened the object at the same time.

‘ He pricked the back of his hand with a pin, when in perfect health, for a drop of blood, which he carefully inspected through his microscope; the exact consistence of which he noted down, to serve for a standard in a scale to measure any future aberrations from, upon the access of any future illness. Thus prepared, when for example, he found it necessary upon the approach of some disorder, to inspect the state of his blood, he pricked his hand as before for a drop, which he carefully compared with the note of his former standard, from which he formed his indication of cure. If he found that the *crassamentum* exceeded in its proportion, he lived pretty much upon weak sops, tea, weak coffee, lemonade, imperial, and the like; and, on the contrary, when he observed an excess in the *serum*, he thickened it by drinking a decoction of Peruvian bark, and a glass of Port now and then. And I make no doubt, had the virtues of Priestley’s airs been discovered in his time, but that Mynheer, towards the completion of his scheme, by a careful and judicious observation of the blood’s colour also, and its variation in different disorders, would have added to his Phar-

macopœia, the three articles of azotic, hydrogene, and oxygene airs, to remedy all its aberrations in that respect also; for example, if, (as it is natural to suppose in a torpid Dutch habit) the blood should happen to exhibit too much of a dark scorbutic red, he would only have had to snuff up a few nostrils-full, now and then, of oxygene air, to quicken the circulation, and give it a beautiful pink. If, on the other hand, it appeared too pinkey and florid, he would only have had to lower it with *quant. suff.* of azotic or hydrogene, till it should be reduced to its natural healthy standard, crimson.

*‘ That a physician just called to a Patient ought, as soon as he comes down stairs, to inform the Family of the Name of the Distemper.*

‘ This, in which the credit of the apothecary is greatly involved, who from his earlier attendance, may possibly have been rash enough to give it some name, may not unfrequently happen to be a very difficult question; till the doctor has had sufficient time to discover it by a most sedulous attention to its various, and possibly complicated appearances. And it may not unfrequently happen, even after the determination of the disorder, that he may never be able to bring all its variations to answer exactly to any species the very fancifully-inventive class-maker may have established. How much, for example, would one be to be pitied, whose reputation, when desired to look at a sore eye, were to be estimated, and depend upon his being able to specify identically which of old Chevalier Taylor’s 134 disorders of the eye, as delivered in his lectures, the present was; many of them, probably, as difficult to discriminate as any of Captain Cook’s newly discovered rocks and headlands. Moreover, if the distemper be a fever, how often are fevers, which in their access are inflammatory, found to become in their progress nervous; or putrid intermittents to become remittent or continual: and *vice versa*? so that like the man’s

account of the wind at *Maker's Tower*, the physician's account may daily shift and vary. And indeed, after all is over, precisely to distinguish between the peculiar symptoms of the disorder, and those probably owing to the medicines made use of, is no very easy matter; for the putrid water, for example, and corrupted juices discharged by stool in any illness where scammony has been much used, is by no means a proof of the previous existence of those humours; for scammony given to the healthiest person will dissolve the blood into a putrid water, and waste the whole body by its repeated use; the healthy juices being first corrupted by the virulency of the medicine, and then discharged.

‘Lecture-reading class-makers often find great trouble (so there is in splitting a hair) in those distinctions without a difference they make in their minute divisions and subdivisions of fevers; with the view, probably, of appearing sapient to their pupils, rather than any real use they can possibly tend to. Probably the truth may lie between these, and a late writer, who, with an ingenious unassuming diffidence, ventures to prove, that of fevers there exists but one sort only; attributing all the variations in their symptoms to the different idiosyncrasies of patients, seasons, treatment, &c. &c. And what strengthens this gentleman's opinion not a little is the known fact, that two people never had exactly the same symptoms in any fever. Mere speculation, indeed, seldom does any good in physic, or even in philosophy or divinity; had a learned prelate, whose business only it was to prove to us the reality of the demoniac miracle, saved himself the needless pains he took to puzzle himself and his readers, in calculating to a scrupulous nicety the number of devils and half-devils allotted to each pig; our belief, taking the whole in the lump, as the inspired apostle relates it, would have been equally strong, and the bishop would have escaped much derision.

‘ There are very few visitors of the sick who do not kindly press the infirm person, though attended perhaps by a physician or two of great skill, to make a trial, in preference to his present medicines, of some very innocent thing; which, from their own knowledge, they assert has been of the greatest benefit in just such a similar case.

‘ The first mistake of the busy interfering lady or gentleman may be, (and a very dangerous one it is,) in the *similarity* of the case. A second may be, that this innocent thing, except it be purely an article of food, can be of any use at all; none of our choicest medicines being innocent things when improperly applied. To a person of a tender face who must be shaved, can it be any recommendation of a razor, that it is as innocent as a lath, which can never be too keen in the hands of persons well skilled in its management? And if by any innocent thing be meant a medicine slow and weak in its operation; is it certain that the disorder may not gain a march upon it, by this trifling procrastination, and great loss of time; and this innocent nostrum be thus actually converted into a poisonous one, for in many cases *non progredi est regredi*? Another error may possibly be, that the art of physic is either universally innate in mankind; or, at least, is so very easily attainable, that scarcely any body is ignorant of it except physicians themselves. If so, what incorrigibly egregious dunces must the regularly educated physicians be, who, after the best classic education, study physic in each of its numerous branches with every advantage for many years at Oxford or Cambridge, completing the whole with a year’s attendance upon the most reputable professors and hospitals in London; if after all they are not supposed to be even upon a par with the generality of other people, who pretend not to have profited by any of those advantages.

‘ To confess the truth, our brethren the Urine Prophets, and Animal Magnetisers, shew abundantly more good sense than any

of us ; as, without the least expense, they create themselves doctors by inspiration ; without pretending the least assistance from learning, or its equal substitute a Scotch *diploma*. And as long as the fools in this credulous nation continue so greatly to out-number the wise, they will never fail of getting into very lucrative practice, and rapidly making their fortunes.

‘ As government at present perfectly acquiesces with any persons practising physic, who can find people weak enough, and willing to be practised upon ; Oxford seems to have no great desire to concern herself much in the matter, divinity and the civil law being her staple commodities. The thing which affects her interest most is her young sister Cambridge opening a private bridle road, to the left of the old turnpike one, to let in country curates, and undersell to them bachelor of divinity degrees ; without keeping terms ; and without any previous examination into, or knowledge of their learning. Had Oxford been politic enough to have elected Mr. Pitt for one of its representatives, he might probably, in return for that honour, have befriended her by inserting a clause in his act against smuggling, against so clandestine a practice, so injurious to the fair trader, the elder *Alma Mater*. As matters now stand, I know not what my much respected old nurse can do better than to open a similar postern wicket, and, like play houses, after the third act, let in country customers at half-price.’

*That ictical Persons see every Thing yellow.*

‘ Though it be a common poetical allusion to see things with a jaundiced eye ; and though this circumstance enters into the great Sydenham’s definition of this distemper ; I have seen numerous patients ictical to the greatest degree, yet never knew one who did not see objects in their natural colour.’

*That it is necessary to drink plentifully of Water-gruel, or some warm diluting liquor, to assist the operation of a purging draught.*

‘The palpable error here is using that as a mean for promoting its operation, which has a tendency on the contrary to weaken it. When in dropsies a very quick smart purge is intended, care should be taken to forbid the patient drinking any thing which may dilute away its strength. What, in my opinion, may have given the first rise to this practice, must have been an attempt, by a great rush of some mild warm liquor, to dissolve and dislodge such hard aloetic pills, or substances of an acrid, insoluble, adhæsiue quality; which, by sticking fast to some part of an intestine, like shoemaker’s wax, may have caused violent painful gripings by their inflaming, fretting, and penetrating the part. This mode of relieving the patient not having been clearly understood, as to its operation, has continued to be fashionable, gripe or not gripe; even when the cause requiring it did not exist; as in Glauber’s salt, or any other so soluble and miscible with the *gastric*, and *enteric* liquors it had to meet with in the *primæ viæ*.

‘I will risk my reputation in being the first person that ever set his face against a very great absurdity, which has hitherto never been suspected of being such, viz. an absolute necessity of a very strict regimen, and making a wonderful fuss about confinement to one’s warm room, and clothing one’s self much warmer than usual, on the day a purge is taken. Whereas (mercurials alone excepted, whose nature it is to be determined by cold weather with violence upon the glands of the throat) I affirm, that a person living, and clothed as usual only, is much less liable to take cold, and to suffer from it, than upon any other days.

‘I have twenty times swam in the Isis, and so have many others, on the afternoon of those days, on the forenoons of



which we had taken an ounce or two of Glauber's salt in *Holywell water*. And in a course of sea-bathing, I have often directed many, after purging in a morning with sea-water, to bathe in it on the evening of the same day. The reason why no harm can possibly happen from this practice I conceive to be this; that nature never at the same time works by directly contrary ways; never plays fast and loose; never opens the perspiratory and sudatory vessels, and closes them also at the same time.

‘As the purge, by its action upon the intestinal glands, and those of the stomach, and upon all the neighbouring vessels, (as also upon that universally pervading lymph, the existence of which L: Cat says he has discovered,) brings a flux of humours into the intestinal canal; cold, by its constringing all the vessels of the external surface of the body, would most evidently tend rather to promote the flux, and operation of the purge, than check and impede it. And, perfectly agreeable to my doctrine, we find that warm diaphoretics, which, by a copious sweat, determine and throw out the acrid humours from the intestines in violent dysenteries upon the external parts, become the most efficacious medicines of any; *per contra* also, in the most dangerous constipations, when no art or medicine has been able to loosen the bowels and procure a motion, setting the patient to stand naked upon a cold marble hearth, and dashing cold water by pail-fulls against his *abdomen*, has often most happily succeeded; and to the admiration of his friends snatched him as it were from the very jaws of death.’

*That Earths and Boles are of the greatest use in correcting acidities in the Stomach of Infants.*

‘I think them very dangerous; for if there should be no acid juices in their stomachs, these are apt to concrete with the mucous matter lodged there into hard indissoluble masses, so



as to line the stomach with a crust capable of obstructing the gastric liquor's passage into it; and also of closing the orifices of the lacteals, so as to obstruct the passage of the chyle through them into the blood.\*

'Some have endeavoured to controvert this opinion by this exception: After a solution of these earths well triturated, in water (wherein was no acid,) had been made, and passed through filtering paper, a portion of a whitish earthy matter has been found, capable enough of passing through the *lacteals* into the blood. The inconclusiveness of this proof will appear by the consideration, that this *residuum* was supplied solely from earth naturally contained in the water used, and not at all from the earth made use of in the experiment; for if pure water distilled had been made use of, though ever so long triturated and digested with the earth, it would have left no *residuum* at all.

*That the Influenza is a very dangerous distemper, and a new one; never known in this Country till a few years ago; at which time the College, by their circular letters, cried out for help from all quarters; were themselves greatly alarmed; and spread a general terror.*

'It is neither a new nor a dangerous distemper; every person going out of a warm room, and riding, by night, against a sharp easterly fog, who had not prudently guarded his nose and mouth from it by a thin soft handkerchief, must have caught it; and so may any one in such circumstances catch it; whether under the new name of influenza, or the old common one of catarrh.

'The present, styled the age of dissipation, may, in my opinion, have pride and pedantry very properly added to the

\* This is surely fanciful enough! This and a few other instances, which we could select from the present performance, might perhaps not unaptly be added to the author's list of medical and vulgar errors.

*Engl. edit.*

bill. Ask the dirtiest, most ragged child you see playing upon a dunghill, whose child he is ; you are answered, Mr. Such-a-one's. *Master*, formerly a title of eminence, is now worn and assumed by the very lowest of mankind ; there are no *women* to be had at present ; even those at a two-penny puppet-show of a country village, forsooth, are all called the *Ladies*. Though at present we cannot pretend to be advanced in stature above former times ; yet we distance them far in our affectation of dignity, pomp, and consequence. Such is our improvement in philology also, that since a travelled fine gentleman has been pleased to dub a common cold, which seized him in his return from a warm climate over Mount Cenis, with a foreign name of *influenza* ; all our catarrhus colds, amongst our gentry, have assumed the same name and importance ; except amongst serious aged people ; who hardly think it worth while to new-model their language, when they catch it, snivel and drivel it off under its former name. In the commutation of terms, sufficient care is at present taken not to exchange for the worse ; formerly *vomits* and *purges* were in use, they are now superseded by *emetics* and *opening medicines* ; *boils*, which a servant maid was formerly used to poultice and open with a pin, are now a *tumour* matured by *cataplasms*, and to be lanced as an *abscess*. There are now no *apothecaries* ; they are all *surgeons* ; and these all *physicians*. There are now no *ensigns* and *lieutenants* ; they are all *noble captains*. Let a ball, in very sultry weather, be ever so much thronged, nobody now *sweats*, though large drops continually trickle down their faces, and require the constant application of handkerchiefs ; the company only *perspire* freely, though every one knows perspiration to be insensible, and not to be seen. No lazy, greasy, foetid, over-fed gentlewoman, is at present *fat*, though of the full weight and admeasurement of Dolly Crampton, cook at the George ; the lady is only *jolly*.

‘No street-walkers, though half-rotten, confess themselves *infected* ; they are only *injured*. No fribble officer, pale, and

trembling through fear upon being ordered into battle, has now the courage of Sir John S—ck—ing bravely in the face of his country to call it *cowardice*; no, he only complains of having naturally *bad nerves*.

‘These may be allowed to be trifling instances of pedantic folly; but that it should infect a great assembly, whom we were in the habit of formerly looking up to as patterns for style and every ornament of speech, is really lamentable; as we now can have no pleasure in reading the speeches, because we do not understand them. We know what a man, bred a scholar, meant when he said *he would not commit a fault*; but never when he said *he would not commit himself*. We knew what a man meant when he said he was *bound to declare* the truth; but when he said he was *free to declare it*, we lost him again, unless the gibberish of *bound* and *free* meant the same thing, which we well know to be contradictory terms.

*That to cure a fractured patella it is absolutely necessary to bring the fractured ends together, and firmly to secure them in that posture with pasteboards and firm bandages, and a long confinement in bed.*

‘I remember a case brought before the ingenious Mr. Samuel Sharp at Guy’s hospital, of a woman carrying on her head a heavy pail, who by a fall broke both the patellas. This afforded him an opportunity of declaring to his pupils, that he would not set one of them, from the numerous bad consequences he had often seen to attend such practice in others, such as a stiff knee, if not an ankylosis for life, and its very great aptitude again to break; whereas he affirmed that no ill consequence of any kind can happen from leaving it entirely to nature. He therefore earnestly exhorted his pupils to set such a value on their characters in private practice, as to put it out of the power of ill-wishers, to accuse them of gross ignorance, or, what is worse, condemning their patients to a tedious and a painful confine-

ment for the sole lucrative purpose of charging them for an operation and long attendance, while they must in conscience be perfectly convinced that all they do is not unnecessary only, but generally mischievous to a great degree.

*That green and bohea tea are the leaves of one and the same tree ; differing only from the different seasons of gathering them, and the different ages of the tree.*

‘ These trees are of late become so common in our gardens that they are well known to differ remarkably from each other. The bohea is a very tender shrub, which requires a green-house in winter, and is perfectly distinguishable from any other teas by its berries. But the green is a hardy shrub, and will stand the weather in our climate, and live out with our gooseberry trees. The relaxing quality of the infusion of teas proceeds from our drinking it hot, if it be made weak, particularly the green ; for when this is made strong and drank cold, it is a remarkable astringent, and when very strong, proves emetic. The Chinese, who must be the best judges of the qualities of teas, make use of bohea only for their own drinking, of which a sufficient quantity for the whole day’s beverage is made in the morning, and carried out with them to their rice ground, or other employments ; which they always drink cold, and without sugar ; they do not infuse it after our manner, but boil it in a tea-kettle, (using tea-pots only as strainers,) probably the better to evaporate the fine smell of their best teas, which are said to have an intoxicating quality, unless they are kept a year or two before they are used.

‘ A more convincing proof of the deleterious quality of our fine green tea can hardly be conceived than Dr. Percival’s. He confined, by a little weight of lead upon his back, a frog upon the rim of a saucer full of green tea, of the heat and strength which our ladies are accustomed to drink it, in such a manner, that one leg should lie in the tea, which soon became paralytic ; he then, in like manner, tried another leg, which suffered alike ;

and so on with the rest, to that degree that the animal could not make the least use of either of them.

‘The mode of manufacturing their teas is said to be as follows: the leaves, when gathered off the trees, have a caustic oil upon them; to take off which, they infuse them in water for some time; after which they with their hands roll the green tea leaves upon copper-plates, heated by a stove underneath. The small remains of the acid oil, still adhering to them by its corroding the copper, is thought to communicate to it the green colour which the English look upon as the test of its goodness. The bohea not bearing so good a price in Europe, though dearer in China, is rolled on a common table with less care, and afterwards dried by stirring it in heated iron pots, with as little trouble as may be; a little tinge from the iron may probably be one reason for its greater healthfulness, and the cause of its colour; and as it is so cheap, there is not the same temptation to adulterate it. The dealers in London, after buying a lot of green tea, spread it upon a long table, and employ women to pick and separate it into several different sorts, as to the colour and size of the rolls, &c. to which they afterwards give what names they think proper, as hyson, imperial, cowslip, &c. &c. However, towards cowsliping a portion of the former, to enhance its price, one or two drops of *otto* of roses is added to a canister of it, to give it the fine smell it is admired for. What tree, or contrivance, produces what is called souchong, I have not learnt: all we know of it is, that it differs much from bohea, as it has no berries in it, and often makes people sick, whom the bohea perfectly agrees with.

*That the smell of a lime-kiln preserves people from consumptions; and that pregnancy has the same effect.*

‘I fear neither assertion is true; because I knew a woman, of about thirty years of age, who was attended before and after pregnancy by an ingenious physician, in a consumption, who

died of it some months after her delivery, though she was born, and always lived as near as possible to eleven lime-kilns in constant work, during the season, the farthest not 200 yards from her house. An unmarried girl, her next door neighbour, died also consumptive.' *Med. and Chir. Rev.*

*Table of the comparative proportions of the human brain with that of other animals.\**

Animals.	Weight of Body.				Brain.				Proport. to the Body			Authors.
	lb	3	5	gr.	lb	3	5	gr				
Man,	140	—	—	—	4	—	—	—	1	to	35	Haller.
	160	—	—	—	4	—	—	—	1	—	40	—
Boy 6 years old,	80	—	—	—	2	—	28	—	1	—	22	—
An Adult,	146	6	—	—	—	—	—	—	1	—	30	Pozzi.
Galeus Piscis,	25	—	—	—	—	—	2	—	1	—	1200	Redi.
Testudo Terrestris,	2	4	—	—	—	—	—	6	1	—	2240	Caldesi.
Marinus,	79	—	—	—	—	$\frac{1}{2}$	—	—	1	—	5688	Ditto and Redi.
Canis Carcharius,	1000	—	—	—	—	$\frac{1}{2}$	—	—	1	—	12000	Steno.
Ditto,	26	—	—	—	—	—	—	—	1	—	2496	Ditto.
Tunny Fish,	390	—	—	—	—	—	1	—	1	—	37440	Redi.
A Lion,	243	—	—	—	—	5	10	8	1	—	615	Buffon.
A Cat,	—	70	—	—	—	—	7	24	1	—	82	Arlet.
Ditto,	8	—	—	—	—	—	6	32	1	—	155	Pozzi.
A Wolf,	64	—	—	—	—	4	3	4	1	—	230	Buffon.
A Dog,	62	—	—	—	—	—	26	—	1	—	305	Ditto.
Ditto,	—	—	—	—	—	—	—	—	1,154.	1,88.	1,54	Arlet.
A Beaver,	30	—	—	—	—	—	—	—	1	—	290	Parisini.
Ditto,	17	—	—	—	—	—	6	—	1	—	362	Buffon.
An Elephant,	5000	—	—	—	10	—	—	—	1	—	500	—
An Ox,	1500	—	—	—	2	—	—	—	1	—	1154	Redi.
Ditto,	866	—	—	—	—	—	—	—	1	—	866	Buffon.
A Dromedary,	369	—	—	—	—	15	—	—	1	—	216	Ditto.
A Horse,	700	—	—	—	—	28	—	—	1	—	700	Ditto.
Ditto,	400	—	—	—	1	—	—	—	1	—	400	Ditto.
An Ass,	198	—	—	—	—	12	2	43	1	—	60	Ditto.
A Stag,	161	—	—	—	—	11	5	—	1	—	221	Ditto.
A Goat,	19	—	—	—	—	—	25	46	1	—	94	Arlet.
A Sheep,	57	—	—	—	—	—	21 $\frac{1}{2}$	—	1	—	324	Buffon.
A Hare,	—	136	—	—	—	—	7	—	1	—	218	Arlet.
A Rabbit,	—	44	—	—	—	—	2	30	1	—	140	Ditto.
A Domestic Mouse,	—	—	36	—	—	—	—	34	1	—	76	Buffon.
A Common ditto,	—	—	—	324	—	—	—	17 $\frac{1}{2}$	1	—	43	Ditto.
A Dormouse,	—	—	23	—	—	—	—	28 $\frac{1}{2}$	1	—	53	Ditto.
A Goose,	—	103	—	—	—	—	—	144	1	—	360	Haller.
An Eagle,	—	—	—	—	—	—	—	—	1	—	160	Borrich.
A Cock,	—	25	—	—	—	1	—	—	1	—	25	Pozzi.
A Sparrow,	—	—	—	380	—	—	—	21	1	—	15	Ditto.
A Canary Bird,	—	—	—	180	—	—	—	13	1	—	14	Ditto.
Apes,	5	9	—	—	—	—	16	90	1—11	1—23	1—107	Buffon.

\* From Rowley's "Schola Medicinæ Universalis Nova."

*Some Additions to a Paper read in 1790, on the Subject of a Child with a double Head. By Mr. Home.\**

Although the paper here referred to made its appearance so long previous to the first publication of the Medical and Chirurgical Review, yet as the subject of it is not a little curious and interesting, and as the additional remarks here given could not be otherwise well understood, we shall briefly recapitulate the particulars.

The double skull here described was furnished to Mr. Home by a friend in the East-Indies, where the child which was the subject of it was shewn alive. There was nothing particular in the circumstances relating to the parents of the child. At the time of its birth, the woman who acted as midwife, terrified at the strange appearance of the double head, endeavoured to destroy the infant by throwing it upon the fire, where it lay a sufficient time before it was removed to have one of the eyes and ears considerably burnt.

The body of the child was naturally formed, but the head appeared double, there being, besides the proper head of the child, another of the same size, and to appearance almost equally perfect, attached to its upper part. This upper head was inverted, so that they seemed to be two separate heads united together by a firm adhesion between their crowns, but without any indentation at their union, there being a smooth continued union from the one to the other. The face of the upper head was not over that of the lower, but had an oblique position, the centre of it being immediately above the right eye.

When the child was six months old, both of the heads were covered with black hair, in nearly the same quantity. At this period the skulls seemed to have been completely ossified, ex-



cept a small space between the ossa frontis of the upper one, like a fontanel.

Of the superior or inverted head it is observed, that no pulsation could be felt in the situation of the temporal arteries; but the superficial veins were very evident. The neck was about two inches long, and the upper part of it terminated in a rounded soft tumor, like a small peach. One of the eyes had been considerably hurt by the fire, but the other appeared perfect, having its full quantity of motion; but the eyelids were not thrown into action by any thing suddenly approaching the eye; nor was the iris at those times in the least affected; but when suddenly exposed to a strong light, it contracted, although not so much as it usually does. The eyes did not correspond in their motions with those of the lower head; but appeared often to be open when the child was asleep, and shut when it was awake.

The external ears were very imperfect, being only loose folds of skin. There did not appear to be any passage leading into the bone which contains the organ of hearing. The lower jaw was rather smaller than it naturally should be, but was capable of motion. The tongue was small, flat, and adhered firmly to the lower jaw, except for about half an inch at the tip, which was loose. The gums in both jaws had the natural appearance; but no teeth were to be seen either in this head or the other. The internal surfaces of the nose and mouth were lubricated by the natural mucus.

The muscles of the face were evidently possessed of power of action, and the whole head had a good deal of sensibility, since violence to the skin produced the distortion expressive of crying, and thrusting the finger into the mouth made it shew marks of pain. When the mother's nipple was applied to the mouth, the lips attempted to suck.



Mr. Stark, who resided in Bengal at this period, endeavoured to ascertain the mode in which the two skulls were united, as well as to discover the sympathies which existed between the two brains. At the time he saw the child it was supposed to be about two years old. At this period the appearances differed in many respects from those taken notice of when only six months old. The eyelids of the superior head were never completely shut, even when the child was asleep, and the eye-balls moved at random. When the child was roused, the eyes of both heads moved at the same time, but those of the superior head did not appear to be directed to the same object, but wandered in different directions: the tears flowed from the eyes of the superior head almost constantly.

The superior head seemed to sympathise with the child in most of its natural actions. When the child cried, the features of this head were affected in a similar manner, and the tears flowed plentifully. When it sucked the mother, satisfaction was expressed by the mouth of the superior head, and the saliva flowed more copiously than at any other time. When the child smiled, the features of the superior head sympathised in that action. When the skin of the superior head was pinched, the child seemed to feel little or no pain.

When the child was about two years old, and in perfect health, the mother went out to fetch some water; and, upon her return, found it dead from the bite of a *Cobra de Capello*.

The double skull was afterwards sent to Europe, and examined by Mr. Home, who gives the following account of its structure and appearances.

‘The two skulls which compose this monstrous head appear to be nearly of the same size, and equally complete in their ossification, except a small space at the upper edge of the ossa frontis

of the superior skull, similar to a fontanel. The mode in which the two are united is curious, as no portion of bone is either added or diminished for that purpose; but the frontal and parietal bones of each skull, instead of being bent inwards, so as to form the top of the head, are continued on; and, from the oblique position of the two heads, the bones of the one press a little way into the natural sutures of the other, forming a zig-zag line, or circular suture uniting them together.

‘The meatus auditorius in the temporal bone of the superior skull is altogether wanting. The brain of the skull is imperfect in several respects. The foramen magnum occipitale is a small irregular hole, very insufficient to give passage to a medulla spinalis; round its margin are no condyles with articulating surfaces, as there were no vertebræ of the neck to be attached to it. The foramen lacerum in basi cranii is only to be seen on one side, and even there too small for the jugular vein to have passed through.

‘The ossa palati are deficient at their posterior part; the lower jaw is too small for the upper, and the processes are imperfect. The number of teeth in both skulls is the same, and is sixteen.

‘From an examination of the internal structure of the double skull, the two brains have certainly been inclosed in one bony case, there being no septum of bone between them. How far they were entirely distinct, and surrounded by their proper membranes, cannot now be ascertained, but, from the sympathies which were taken notice of by Mr. Stark between the two heads, I should be inclined to believe that there was a more intimate connection between them than simply by means of nerves, and therefore that the substance of the brains was continued into one another.

‘Had the child lived to a more advanced age, and given men of observation opportunities of attending to the effects of this double brain, its influence upon the intellectual principle must have afforded a curious and useful source of inquiry.’—

Mr. Dent’s observations, in addition to those above given, are the following.

The child was a male. Its father was a farmer at Mundul Gaut, in the province of Bardwan, who told Mr. Dent, that it was more than four years old at the time of its death.

The mother, who was thirty years of age, had three children, all naturally formed, and her fourth child was the subject of the present paper. Mr. Dent endeavoured to discover whether any imaginary cause had been assigned by the parents for the unnatural formation of the child; but the mother declared, that no circumstance whatever, of an uncommon nature, had occurred: she had no fright, met with no accident, and went through the period of her pregnancy exactly in the same way as she had done with her other children.

The body of the child was uncommonly thin, appearing emaciated from want of due nourishment.

The neck of the superior head was about four inches long; and the upper part of it terminated in a hard, round, gristly tumour, nearly four inches in diameter.

The front teeth had cut the gums, in the upper and under jaws of both heads.

When the child cried, the features of the superior head were not always affected: and when it smiled, the features of the superior head did not sympathise in that action.

In preparing the skull, which unpleasant operation Mr. Dent was obliged, from the prejudices of his servants, to superintend, he found that the dura mater belonging to each brain was continued across, at that part where the two skulls joined, so that each brain was invested in the usual way by its own proper coverings; but the dura mater which covered the cerebrum of the upper brain, adhered firmly to the dura mater of the lower brain; the two brains were therefore separate and distinct, having a complete partition between them formed by an union of the duræ matres.

When the contents of the double skull were taken out and the union of the duræ matres more particularly examined, a number of large arteries and veins were seen passing through it, making a free communication between the blood vessels of the two brains. This is a fact of considerable importance, as it explains the mode in which the upper brain received its nourishment.

Before these observations were communicated by Mr. Dent, it was natural to suppose that the two brains had been united into one mass; as it was difficult to imagine in what way the upper brain could be supplied with blood.

*Med. and Chir. Rev.*

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*On the whitening of Bones.\**

An ingenious paper on the whitening of bones, is furnished by Mr. Richard Smith, of Bristol. ‘In the spring of 1798,’ Mr. Smith observes, ‘I was struck with the wonderful effects produced by the bleaching liquor of cotton and linen manufac-

\* From Beddoes’ “Contributions to Physical and Medical Knowledge.”

turers. It immediately occurred to me, that its action upon bones was worthy of investigation; and a favourable opportunity of trying it soon after presenting itself, I was so convinced of its efficacy, that I immediately fitted up an apparatus for the purpose.

‘I had at this time a cranium in my possession, lately prepared, perfectly clean, and inodorous, but so brown, that I had thrown it aside, as unworthy a place in the museum. This (together with the vertebræ of the neck) formed the subject of my first experiment. It was evident, that every good purpose might be answered by exposing the bones to an oxygenated muriatic atmosphere merely; and in order to accomplish this, I adapted a cork with a stop-cock driven through it, to the aperture made for the candle, at the bottom of a common lamp-glass. A Florence-flask, which contained a quarter of an ounce of *black calx of manganese, mixed with half an ounce of muriatic acid*, served for a retort, and a long-bent tube of glass fixed into its neck, enabled me to direct the product at pleasure.

‘The skull had been immersed for twelve hours, in a weak caustic solution of potash; but whether this is absolutely necessary, I have not yet ascertained.

‘The lamp which contained the head and several other bones, previously prepared in the same manner, was placed in a large trough of water. The stop-cock at the end, afforded me the means of filling it with water, by exhausting the column of air above. Every thing being thus in readiness, it remained only to dislodge the water, by placing the flame of a spirit-lamp under the retort, and the extremity of the tube under the lamp-glass. In a few minutes, the yellow oxygenated muriatic acid gas, was evolved, and I perceived, that as the water sunk, those bones which were exposed to its influence, assumed the same appearance.

‘The operation was continued till the glass was emptied; but I found it necessary to continue the process slowly, in order to recruit the air which had been absorbed by the preparation and water; a circumstance which was demonstrated by the rising of that fluid.

‘In six or eight hours I removed the glass, and exposed the head, which was of a bright golden colour, to the open air, and the rays of the sun: as it dried it became paler, and covered with small shining crystals (which were afterwards washed off in rain water), and I had soon the pleasure to find it so beautifully white, that it exceeded in appearance every thing of the kind which I before possessed. The dark case in which it has been some time enclosed, has rather diminished its whiteness; a circumstance which usually takes place where the light is excluded; but it still retains its beauty sufficiently to demonstrate the superiority of this process.

‘With old bones it does not succeed so well as with recent; but even these, in some cases, were well bleached.’

*Med. and Chir. Rev.*

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*Alyon's Oxygenated Ointment.*

Sixteen parts of fresh lard are melted in a glazed earthen vessel, with a moderate degree of heat, and when melted, two parts of pure nitric acid of  $32^{\circ}$  are poured upon it; the heat is maintained till the mixture boils, when the vessel is removed from the fire, and permitted to cool. In this operation the nitric acid is completely decomposed. Two ounces of fat, treated with two drachms of acid, acquired one drachm in weight, and pure

azotic gas was collected in a proper apparatus. Although the process seems extremely simple, it requires considerable dexterity to form an ointment always similar in its effects and appearance. The greatest difficulty is, to catch the moment when the decomposition of the acid is completed; for the smallest increase of heat at that time, disunites its principles, and separates a quantity of carbon.

When well prepared this ointment has no taste, and yields to the action of water neither nitric nor sebatic acid; is of a yellowish-white colour, of a consistence between suet and virgin wax, melts without alteration, and facilitates the oxydation of metals rubbed with it. If the acid be in excess, the ointment will not become firm, and its use will inflame the skin; if it be in too small a quantity, it will be less active.

Fat may be made, M. Alyon observes, to decompose a greater quantity of acid, by adding it in different portions. The ointment then has little smell, is of a higher colour and firmer consistence, and spreads easily on linen. This is preferable in diseases of the skin, in herpetic and venereal ulcers, and may be usefully employed in healing wounds, and atonic ulcers, while the less oxygenated ointment is better adapted for diseases of the face.

*Med. and Chir. Rev.*



*An Account of the Pelagra, a Disease endemical in the Dutchy of Milan.\**

This account is extracted from a treatise on the subject, written some years ago, by Dr. Jantèn, of Leyden. He describes this curious disease as follows:—

\* From the Lond. Med. Rev. and Magazine.

“ About the month of March or April, when the season invites the farmers to cultivate their fields, it often happens, that a shining red spot suddenly arises on the back of the hand, resembling the common erysipelas, but without much itching or pain, or indeed any other particular inconvenience. Both men and women, boys and girls, are equally subject to it. Sometimes this spot affects both hands, without appearing on any other part of the body; not uncommonly it arises also on the shins, sometimes on the neck, and now and then, though very rarely, on the face. It is also sometimes seen on the breasts of women, where they are not covered by the clothes, but such parts of the body as are not exposed to the air, are seldom affected; nor has it ever been observed to attack the palm of the hand, or sole of the foot. This red spot elevates the skin a little, producing numerous small tubercles of different colours; the skin becomes dry, and cracks, and the epidermis sometimes assumes a fibrous appearance. At length it falls off in white furfuraceous scales, but the shining redness underneath still continues, and in some instances remains through the following winter. In the mean time, excepting this mere local affection, the health is not the least impaired, the patient performs all his rural labours as before, enjoys a good appetite, eats heartily, and digests well. The bowels are generally relaxed at the very commencement of the disease, and continue so throughout its whole course. All the other excretions are as usual, and, in females, the menses return at their accustomed periods, and in the proper quantity. But what is most surprising, is, that in the month of September, when the heat of the summer is over, in some cases sooner, in others later, the disorder generally altogether disappears, and the skin resumes its natural healthy appearance. This change has been known to take place as early as the latter end of May or June, when it has only been in its earliest stage. The patients, however, are not now to be considered as well: the disease hides itself, but is not eradicated. For, no sooner does the following spring return, but it quickly



re-appears, and generally is accompanied with feverer symptoms. The spot grows larger, the skin becomes more unequal and hard, with deeper cracks. The patient now begins to feel uneasiness in his head, becomes fearful, dull, less capable of labour, and much wearied with his usual exertions. He is exceedingly affected by changes in the atmosphere, and impatient both of cold and heat. Nevertheless he generally gets through his ordinary labour, with less vigour and cheerfulness indeed than formerly, but still without being obliged to take to his bed; and as he has no fever, his appetite continues good, and the chylopoiëtic viscera perform their proper functions. When the pelagra has even arrived at this stage, the returning winter, nevertheless, commonly restores the patient to apparent health; but the more severe the symptoms have been, and the deeper root the disease has taken, the more certainly does the return of spring reproduce it with additional violence. Sometimes the disease in the skin disappears, but the other symptoms remain notwithstanding. The powers both of the mind and body now become daily more enfeebled; peevishness, watchings, vertigo, and, at length, complete melancholy, supervene. Nor is there a more distressing kind of melancholy any where to be seen, than takes place in this disease.—‘On entering the hospital at Legnano,’ says Dr. Jansen, ‘I was astonished at the mournful spectacles I beheld, especially in the women’s ward. There they all sat, indolent, languid, with downcast looks, their eyes expressing distress, weeping without cause, and scarcely returning an answer when spoken to; so that a person would suppose himself to be among fools and mad people, and, indeed, with very good reason; for gradually this melancholy increases, and at length ends in real mania. Many, as I had an opportunity of observing in this hospital, were covered with a peculiar and characteristic sweat, having a very offensive smell, which I know not how better to express than by comparing it to the smell of mouldy bread. A person accustomed to see the disease would at once recognize it by this single symptom. Many complained

of a burning pain at night in the soles of the feet, which often deprived them of sleep. Some are affected with double vision, others with fatuity, others with visceral obstructions, others with additional symptoms. Nevertheless fever still keeps off, the appetite is unimpaired, and the secretions are regularly carried on. But the disease goes on increasing, the nerves are more debilitated, the legs and thighs lose the power of motion, stupor or delirium come on, and the melancholy terminates in confirmed mania. In the hospital at Legnano I saw both men and women in this maniacal state. Some lay quiet; others were raving, and obliged to be tied down to the bed, to prevent them from doing mischief to themselves or others. In almost all these the pulse was small, slow, and without any character of fever. One woman appeared to have a slight degree of *furor uterinus*, for at the sight of men she became merry, smiled, offered kisses, and by her gestures desired them to come towards her. Some were occupied in constant prayers, some pleased themselves with laughter, and others with other things. But it was remarkable, as Moscati observed, that all who were in this stage of the disease had a strong propensity to drown themselves. They now begin to grow emaciated, and the delirium is often followed by a species of *tabes*. A colliquative diarrhoea comes on, which no remedy can stop, as also has been observed in nostalgia. Sometimes in the pelagra the diarrhoea comes on before the delirium, and the delirium and stupor mutually interchange with each other. The appetite often suddenly fails, so that the sick will sometimes go for near a week without tasting food. Not uncommonly it returns suddenly, so that they eagerly devour whatever is offered them, and this even at times when they are horridly convulsed. The convulsions with which they are attacked are most shocking to see, and are of almost every kind, catalepsy excepted, which has been described by writers. I saw one girl in bed, who was violently distorted by opisthotonos, and others with every species of tetanus. At length syncope and death close the trage-

dy, often without any symptom of fever occurring through the whole course of the disease."

It appears that this disease is not infectious, nor is the cause producing it ascertained. Dr. Jansen seems to place little or no reliance on any plan of treatment which has been hitherto employed. An account of the appearances on dissection is given in a subsequent part of the volume; but it throws no light on the nature of the affection.

*Med. and Chir. Rev.*



*On the Solution of Caoutchouc.*

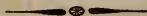
It is sufficiently understood in general, that the proper and only effectual solvent of the *caoutchouc*, or elastic gum, is æther: yet vitriolic æther, as it is usually procured, does not dissolve it so perfectly, as to make it applicable to various purposes for which it is fitted. To render æther a perfect solvent of the *caoutchouc*, it is necessary it should undergo the following process, the invention of Mr. Winch, apothecary in London. A pound of æther is to be put into a bottle capable of containing about four pounds of any common liquid. On this are poured two pounds of pure water: the bottle is then stopped, held with the mouth downwards, and strongly shaken, in order to mix the two liquors. On discontinuing the shaking, the æther soon swims uppermost; the bottle is still held in the same position, and cautiously opened, keeping the thumb on the mouth of it. The water is by this means easily let off into a vessel below. The same operation is to be repeated two or three times, with fresh quantities of water until the sixteen ounces of æther are reduced to about five ounces. It is this purified æther that is found to be the most perfect solvent of elastic gum, which is to be thrown into the æther, after being cut into small pieces.

The elastic gum begins to swell in a very short time; and at the end of five hours, or longer, the liquor is saturated, and remains transparent. If there be a surplus of elastic gum, it subsides to the bottom, and on being taken out of the liquor, may be moulded into any form, and will preserve its elasticity.

To form a tube (or other shape) of elastic gum, Mr. Cavallo employs the following method. A small cylinder of pipe-clay is first prepared, of the diameter and length of the intended tube. It is not to be baked, but simply to be dried. The æther saturated with gum, is poured into a case of glass, or tin, which should be a little longer than the clay cylinder; this is filled up to the brim. The operator then plunges the whole length of the clay pipe into the æther, withdraws it suddenly, lets it remain for an instant in the air, re-plunges it anew, and repeats the operation in proportion to the intended thickness of the tube; for each immersion and evaporation produces a small coating. This being done, the clay cylinder, covered with elastic gum, is plunged into a vessel of water; the mould of clay is there speedily dissolved, and the gum remains in the state of a perfect tube.

The water used in washing the æther should be preserved, as a part of the æther mixed with it may be recovered by distillation.

*Med. and Chir. Rev.*



*Process followed in Suabia, for making the essential Salt of Sorrel.\**

This salt, which is employed for the purpose of taking iron-moulds, and spots of ink, out of linen, is likewise very com-

\* The salt sold in England for the purpose of taking ink-spots and iron-moulds out of linen, &c. and which is called *essential salt of lemons*, is prepared from sorrel, in the manner mentioned above.

monly used in France as a substitute for the juice of lemons, (to which it is not at all inferior in flavour, or in wholesomeness,) in acidulated drinks, and other things of like description. The chief part of this substance is procured from Germany, from that canton which is called the *Black Forest*. There it is prepared by the peasantry, who carry it to the town of *Basle*, whence it is distributed to most parts of the world. The following is the process employed in the preparation of this salt, as communicated by M. *Baunach*, chief apothecary to the military hospital at *Brest*.

This salt is chiefly procured from that species of sorrel, known by botanists under the denomination of *rumex acetosa foliis sagittatis* (Lin.) The seeds of the plant are sown in extensive fields in the month of March. It grows vigorously, and is fit for cutting in June. When cut, it is immediately deposited in a proper mill, where it is sufficiently bruised, and the juice and pulp diluted with a quantity of water, and suffered to stand for some days, when the whole is submitted to the press, much in the manner that grapes are managed in the manufacture of wine.

The juice of the herb, however abundant it may seem, is by no means sufficient for holding in solution all the essential salt contained in the plant; for which reason water is added, as we have seen; the part remaining after pressure is again moistened with a quantity of water, as before, and again pressed; and the operation is thus repeated three or four times, or as long as the plant is found to contain any extractive matter.

The whole of the juice, thus collected, being deposited in a tub, or other proper vessel, some water is added, in which a quantity of pure white argillaceous earth is diffused (twenty pounds of clay are usually put to twelve hundred pints of the juice); it is left to settle for twenty-four hours, and then decanted, the remainder being repeatedly washed and filtered

through woollen cloth. The juice thus clarified is evaporated by a gentle heat, till a pellicle forms on the surface, when it is set by, in proper vessels, for a month, to crystallize. The remaining liquor is subjected to a second and third evaporation and crystallization, till no more of the essential salt is afforded. The mother lye which remains is found to contain a considerable quantity of the *salt of silvius*, together with a small portion of vitriolated tartar; it likewise effervesces with alkalies.

The crystals thus obtained by the different operations, are purified by a further solution in water, filtration and evaporation; when pure white crystals are procured for use. The other species of sorrel afford a salt of the same kind.

From a pound weight of the leaves treated as above, may be obtained as follows:—

One drachm of pure essential salt.

Four grains of *salt of silvius*.

One quarter of a grain of vitriolated tartar; and

Four ounces of extractive matter.

*Med. and Chir. Rev.*

*from Bayen's Opuscules Chymiques.*

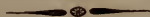
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#### *Method of purifying Molasses.*

M. *Devaux*, of Petersburg, communicates a method of purifying molasses from its sharp taste; viz. by mixing equal quantities of molasses and water, with an eighth part of powdered charcoal, and boiling them together for half an hour over a

moderate fire. On cooling, the charcoal subsides, and the superfluous water is afterwards dissipated by evaporation.

*Med. and Chir. Rev.*



*Acid of Sugar formed on Scrophulous tumours by immersion in Spirit of Wine.*

Mention is made in *Hufeland's* journal, of a curious fact, that may possibly throw some light on the nature of scrophula. Some scrophulous tumours being immersed, for the purpose of preservation, in spirit of wine, their surfaces were, after some time, found covered with fine prismatic crystals, which, on being submitted to the proper chemical tests, were found to consist almost wholly of the acid of sugar.

*Ibid.*



*Method of destroying the offensive Smell of Privies, Night-Chairs, &c.*

The following is given, as a cheap and effectual means of destroying the offensive smell of privies, night-chairs, &c. an object of no small consequence both in regard to health and comfort. If a certain quantity of milk of lime (water in which lime has been recently slaked, and poured off previous to its settling) be mixed with a ley of ashes, or even soapy water that has been used for washing, and thrown into the sink of a privy, or other convenience for the sick room, it will immediately destroy the offensive odour. It will readily be conceived, that the purpose may be more simply answered, by mixing together a few



pounds of quick lime, a small quantity of wood-ashes, and a bucket of water.

*Med. and Chir. Rev.*

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*Efficacy of cold Water in Hernia.*

M. *Lombart*, first surgeon at Rethel, communicated to the society of Medicine at Paris, an account of the good effects experienced from compresses moistened with cold water, and applied immediately to the surface of the tumefied intestines, after the operation for hernia had been performed. He was by this practice enabled to reduce with facility the protruded intestine, after having in vain attempted it by the ordinary means, and even by enlarging the dimensions of the wound.

*Med. and Chir. Rev.*

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*Sheldon, on the Preservation of anatomical Subjects.*

The following is said to be Mr. John Sheldon's method of preserving, entire, anatomical subjects, and which it effects in a very surprising manner. He injects several parts of the body with strong spirits of wine, saturated with camphire, and mixed with a small quantity of turpentine. The skin is prepared with finely-powdered alum, rubbed on with the hand. The intestines are taken out, and covered with a varnish composed of a mixture of camphire and common resin. All the internal parts of the body undergo the same operation, and are afterwards rubbed with alum. The body, thus prepared, is laid on a bed of calcined chalk to the thickness of an inch, in order to absorb all humidity, and is then placed in a double case of wood, the inner of which is of cedar. In this manner Mr



Sheldon has been able to preserve for several years the body of a young woman, which retains its form and appearance nearly as perfect as in life. Even the natural tint of the skin of the face is preserved, by a coloured injection impelled through the carotids to produce that effect.

*Med. and Chir. Rev.*

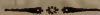


*Biliary Concretion voided by Stool.*

A case of biliary concretion voided by stool, is communicated by Mr. Woollett, of Monmouth. The patient had been subject to symptoms indicating the presence of gall-stones for twenty years, before he voided the one here mentioned. It resembled a kidney in shape, its greatest circumference being five inches and two-thirds: its weight seventeen scruples, eight grains. The patient continued well afterwards.

*Med. and Chir. Rev.*

*from Lond. Med. Rev. and Magazine.*



*Hyosciamus in Hooping-Cough.*

The *hyosciamus* is strongly recommended in the treatment of hooping-cough. Of the extract of this plant, ten grains were dissolved in two drams of antimonial wine. Infants of a year old took about two grains in twenty-four hours, augmenting the dose according to age. When the fit of coughing was exceedingly violent, and threatened suffocation, it was often terminated by exciting vomiting, by means of the finger, or a feather dipped in oil, and thrust into the fauces. When the inflam-

matory symptoms had subsided, and emetics had been premised, the Peruvian bark was exhibited with much advantage. In cases of worms, *ox-gall*, given in the form of pills, is highly spoken of, both as an anthelmintic and antispasmodic.

*Med. and Chir. Rev.*

*from Brewer's Bibliotheque Germanique.*



*Ascarides passed from the Bladder of Urine.*

The second number of the *Bibliotheque Germanique* commences with a case of *ascarides*, passed from the bladder along with the urine; as related by Dr. Kuhn, physician at Eisenach. (*Gazette Medicale Allemande, May, 1795.*) The most common seat of worms in the human body is, in those situations which have a communication, more or less direct, with the external air, as the intestinal canal. But they have been found likewise, in situations, which it is difficult to account for their arriving at; this is the case with the urinary bladder, from which different persons have observed them issue, both the *round-worm*, the *hydatid*, and also, the *ascaris*; of which last, an instance is here furnished, in a child, six years of age, who, after suffering severely, for some hours, cataleptic symptoms, passed with his urine above two hundred living *ascarides*: after which the symptoms disappeared, without recurrence. None of the worms were passed by stool, though strong purgatives were administered.

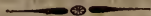
*Med. and Chir. Rev.*

*from the same.*

*Two Cases of the successful Termination of Wounds, hitherto deemed incurable.*

The first of these was a division of the internal jugular vein, in consequence of the extirpation of a large tumour seated on the left side of the neck, and which resembled cancer. This passed so deep as to form a connexion with the internal jugular, which was necessarily divided, but the bleeding from which was readily restrained by ligatures. The parts afterwards healed in the usual way. The second case was of a wound in the uterus, which, though classed amongst mortal ones by the older writers, has many exceptions: a woman had laboured under *ascites* for several years, for which the *paracentesis* was performed, with relief of the symptoms. On a second collection taking place, the operation was repeated; when, instead of water, a discharge of blood followed to the amount of six ounces: the blood ceased to flow on withdrawing the canula. It turned out that the woman was five months gone with child at the time; and she was delivered of a healthy infant at the usual period, having suffered no inconvenience from the operation. She was afterwards again tapped for a further accumulation.

*Med. and Chir. Rev.*  
from *Med. Facts and Obs.*

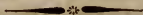


*Smyth's flexible metallic Bougies and Catheters.*

We mentioned in a cursory way, a short time ago, the invention of flexible metallic bougies and catheters, by Mr. Smyth, apothecary, of *Tavistock street*. Having since had an opportunity of examining them, and of witnessing their application, we are enabled to speak more decidedly of their merits. They ap-

pear to be equally flexible with the common plaister bougie, without the inconvenience of being readily broken, or yielding too much, from the heat of the parts to which they are applied. At the same time, they possess sufficient firmness for any degree of force, which it can be proper to make use of, in overcoming an obstruction mechanically; and they are readily susceptible of a very high polish. When these properties are considered, together with their durability, as with moderate care they may last for many years, we have no doubt they will be considered as an important and valuable discovery.

*Med. and Chir. Rev.*



### *Uncommon Doses of Zinc in Epilepsy.*

‘Another case which occurs to my memory as worthy of medical record, from the uncommon violence of the disease, and the uncommon quantity of zinc given to subdue it, is one of epilepsy, by much the most alarming I ever met with, in thirty years practice. The paroxysms returned four times in twenty-four hours, with wonderful violence; whilst each fit was accompanied by a most distressing tetanus, that drew the patient’s head backwards in a frightful manner, and rendered it impossible to keep him in bed, without using such violence as threatened the dislocation of some member; so that we were obliged to let him roll about on the floor of a room spread with mattresses.

‘The young sufferer was a gymnast of the imperial corps of noble cadets, aged sixteen, who was carried into the lazaret, or hospital of the establishment, in the deplorable state mentioned above; brought on, as we were informed, by a fright sometime before: but his mother, a poor widow, had concealed his

being subject to the disease, lest he should have been refused admittance into the corps.

‘As the case was highly alarming, nature being unable to sustain long so violent a conflict with scarce any intermission, I formed the resolution of giving the flowers of zinc with empiric boldness; and, after ordering ten ounces of blood to be taken from the arm, as the youth was plethoric, I gave him eight grains of *flor. zinc.* the first day, with conserve of roses, and augmented the dose by four grains every fourth day, till the thirty-second from the attack; when it amounted to two scruples, or forty grains, which he took for a month consecutively, till every vestige of the disease was gone.

‘No other medicine was found necessary during the whole cure, as the zinc kept his body sufficiently open; and it was highly interesting to observe, that, in proportion as the dose of the sedative mineral was augmented, the disease gradually diminished in frequency and violence; first, to three fits a day; then to two; next, to one; till, on the thirty-second, the dose of two scruples completely overcame the spasms, and the disease finally vanished, never, I hope, to return, as he has now been two years without a single fit.

‘The distressing tetanus diminished likewise in violence with the paroxysms of epilepsy, though it never left our young patient till the whole ceased together at the epoch mentioned above, although I thought it prudent to continue the large dose of two scruples for a whole month, to ensure the permanence of the cure, as no disagreeable consequences ever attended the exhibition of the zinc, except a little trifling nausea towards the beginning, which went off without giving us any trouble. I must not forget to remark, that the dose was alway divided into two equal parts; one to be taken in the evening, the other in the morning.’

*Med. and Chir. Rev.*

*from Duncan's Annals.*

*Experiments on the gaseous Oxide of Azote, by a Society of Amateurs, at Toulouse; communicated by M. P. DISPAN, Professor of Chemistry in the college of that city, to the Editors of the Annales de Chimie.*

The different, and even contradictory results, which have been published, from time to time, respecting the effects produced by the respiration of the gaseous oxide of azote, led to the following experiments, which were performed on more than a dozen persons, and in some cases repeated two or three times on the same individual. The sensations experienced by each; were carefully noted down at the moment, and the following account is drawn from these memorandums.

The nitrate of ammonia employed in these experiments, was indistinctly crystallized, but perfectly neutral. Its taste was extremely pungent, with a slight odour. It had been formed by saturating very pure nitric acid with ammoniacal gas obtained from the distillation of sal ammoniac with the common pot-ash of the shops.

About 1545 grains of this salt were put into a small retort, and placed on a sand bath; the salt melted and boiled for a short time without yielding any gas, but at length the retort became filled with a white vapour, which however quickly disappeared; after which the gas was rapidly disengaged and received in bladders.

The same process was performed on a larger scale, 10 oz. troy of the salt being employed, from which a sufficient quantity of gas was obtained to fill eight bladders. In this last case, a red vapour arose within the retort as it began to cool, which was ascertained by experiment to contain no nitrous gas.

The gaseous oxide of azote, when tasted or inhaled, is generally allowed to possess a strong saccharine taste, which fre-

quently remains on the organs during the whole day after receiving it. M. Dispan observed in it an after-taste of nitre, but acknowledges that it was the last collected gas which he tasted. Another gentleman belonging to the Society, perhaps under a similar impression, says he perceived in it an astringent quality.

In these experiments the gas was respired by means of a bladder furnished with a stop-cock, which was applied to the mouth; the nostrils being closed, and the lungs emptied as much as possible.

The first person upon whom the experiment was tried, swooned at the third inspiration, and remained senseless about five minutes, when he recovered, but with a sensation of great lassitude and fatigue. He recollected to have experienced only a sudden faintness, attended with a tingling at the temples.

The second, M. de M. observed a saccharine astringent taste; he experienced a sense of great dilatation, accompanied with heat in the breast; his veins swelled, his pulse quickened, and he became vertiginous. But he thought he could have borne a stronger dose, the bladder not being large enough for his lungs.

The third experienced a saccharine taste on the first inspiration, but was insensible to those which succeeded; his lungs were forcibly dilated with great heat. When the bladder was removed, he appeared very comfortable, but could not refrain from violent bursts of laughter.

The fourth individual subjected to this experiment experienced the same saccharine taste as those who preceded him, and he retained the impression from ten o'clock in the morning till past midnight. His legs trembled under him, and he became frequently vertiginous during the remainder of the day.



The fifth perceived the same saccharine taste. On resigning the bladder he was seized with dizziness, succeeded by a sensation of great pleasure throughout the body; he felt likewise a sense of great weakness in his lower extremities.

The sixth subject of this experiment perceived the saccharine taste throughout the remainder of the day; his ears tingled, his legs tottered, his stomach was oppressed, and altogether his sensations were rather painful than agreeable.

With the view of ascertaining if the mode of breathing from a bladder might produce any influence on the foregoing results, the same individuals were requested to inspire atmospheric air in the same manner. They were all fatigued by it, but nothing more. The bladders were next filled with oxygen gas; and the only difference between it and common air was, that it produced an increase of heat in the lungs. The singular effects, therefore, which are described above, can only be attributed to the gaseous oxide of azote.

At a subsequent meeting of the society, other and more extensive experiments were entered on.  $27\frac{1}{2}$  ounces troy of nitrate of ammonia, prepared as before, were put into a retort, with its neck fitted to a double-bodied receiver; from whence, by means of a tube, the gas passed into a vessel inverted over water. The retort was placed on a sand-bath. As soon as the heat affected the retort, the salt melted; and nearly at the same moment sparkling vapours arose in the retort, but in a very small quantity. The air which the heat expelled from the vessels had a nitrous odour; but this, as well as the vapours, gradually diminished; as the process continued, they disappeared however altogether, and were succeeded by a lively smell of Prussic acid. At length the retort became filled with white vapours, and the gaseous oxide of azote began to pass over. The disengagement soon became so abundant, that it was judged proper to withdraw the fire; but afterwards, on replacing the



coals, the gas which had diminished in the interval, was again so rapidly evolved, that the luting of the vessels began to give way; yet, notwithstanding the loss thus occasioned, the disengagement continued extremely rapid in the receiver for at least a quarter of an hour.

M. Dispan supposes, that an explosion was only prevented in this case by the luting having given way. He next proceeds to state the effects which were produced by the respiration of the gas. Twelve persons were subjected to the experiment, and on many it was repeated. Most of them had inhaled the gas on the former trials, when two out of seven experienced pleasing sensations, but on the present occasion not one felt pleasure; on the contrary, they all felt pain, and many of them suffered severely. One person in particular stamped with his foot during the whole time of breathing the gas. When the bladder was removed, he recovered from the profound stupor into which he had been thrown, and complained of a pain in the back part of his head, as if he had received a violent blow from a dagger; his sensations were so painful that he could not be prevailed on to make a second trial. The other individuals were in general affected with vertigo and dizziness, succeeded in some by involuntary convulsive fits of laughter.

M. Dispan's description of the effect of this gas upon himself is thus recorded. "At the first inspiration," says he, "I emptied the bladder, on which I felt a saccharine taste in my mouth, and my lungs immediately became inflated. I emptied and filled them again; but upon the third attempt, my ears were affected with a tingling noise, and I dropped the bladder. I did not, however, wholly lose my consciousness, but remained in a kind of benumbed astonishment, rolling my eyes about at random. I was then suddenly seized with convulsive fits of laughter, such as I had never before experienced; in a few seconds, however, this propensity to laugh suddenly ceased, and I no longer felt any unpleasant symptoms."

Two others, on whom the gas was tried, experienced only a convulsive motion of some of the muscles of the face; but were, in the course of the day, attacked with violent diarrhœa.

M. Dispan is of opinion, from every thing he observed, that it will be extremely difficult to reduce the effects of gaseous oxide of azote to any general system, as they vary so considerably on different individuals: and, what is more singular, even on the same person.

This paper is concluded with an account of some experiments made by M. D. in order to ascertain the effects of gaseous oxide of azote upon animals. He placed a green-finch in a vessel of sufficient dimensions, and filled it with the gas in question. At first, the bird seemed to suffer no inconvenience, but he soon gradually closed his eyes, and dropped gently on his side, as if asleep. On being restored to the pure air, he resumed his feet, without however attempting to fly away. About an hour afterwards, he was subjected to a second trial, and having been suffered to remain longer in the vessel, he was taken out quite dead. M. Dispan thinks it very remarkable, that the bird should make no effort to escape, or that he should manifest no convulsive symptoms, such as take place in experiments with other gases.

*Med. and Phys. Jour.*



## MEDICAL COMMENCEMENT.

At a Medical Commencement held in the University of Pennsylvania, on Friday April the 10th, the following gentlemen were admitted to the degree of Doctor in Medicine, having defended the Theses attached to their names.

George Cheyne Shatluck, of Massachusetts. On Hydrocephalus internus.

Thomas Bryant, of Pennsylvania. On Tetanus.

Samuel B. Smith, of Pennsylvania. On the means of preserving health in youth and old age.

- Henry Neill, of Pennsylvania. On Bubonocoele.  
Alexander Knight, of Pennsylvania. On Vaccination.  
James Glen, of Pennsylvania. On Dysentery.  
Edward Lowber, of Pennsylvania. Inflammation of the viscera in yellow fever.  
William Baldwin, of Delaware. On the diseases which appeared in a voyage to India.  
Edward Anderson, of Maryland. On the *Rhododendron maximum*, and punctatum.  
James Thomas, of Maryland. On the cause of inflammation in wounded cavities.  
William Hoomes of Virginia. On Puerperal fever.  
James M'Dowell, of Virginia. On the *Phytolacca decandra*.  
Robert Miller, of Virginia. On the sedative effects of Cold.  
James Kello, of Virginia. Against the vitality of the blood.  
David Moore, of Virginia. On Ophthalmia.  
Elisba Clark, of Virginia. On *Æsculus pavia*, *lutea*, and *spicata*.  
Peter T. Beally, of Virginia. On *Epigea repens*, and *Gaultheria procumbens*.  
James Minor, of Virginia. On Concussion of the brain.  
Thomas B. H. Grey, of Virginia. On *Cynanche trachealis*.  
George Thornton, of Virginia. On the ameliorating effects of medicine.  
William R. Nelson, of Virginia. On Apoplexy.  
William Steptoe, of Virginia. On Animal sympathy.  
Charles B. Robinson of Virginia. On Dysentery.  
Peter Custis, of Virginia. On Absorption.  
John Gilmer, of Virginia. On the Bilious fever which appeared in the county of Albermarle, in the year 1806.  
Daniel Dobbins, of Virginia. On the Scurvy.  
Cornelius Dupont, of South Carolina. On Digestion.  
Samuel W. Ferguson, of South-Carolina. On the sedative effects of cold.  
John Ramsay, of South-Carolina. On Catarrh.  
Baron John de Bretton, of the island of St. Croix. On Menstruation.  
Richard Brown. On the application of physiognomy to medical science.
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*The following Facts are laid before the Public for the encouragement of those, who entertain any doubt respecting the efficacy and success of Vaccine Inoculation; and to confirm others in their good opinion of that inestimable blessing.*

In March, 1800, having previously informed myself of the safety and efficacy of the Cow-Pock, I began to inoculate my

two parishes *Leckhamstead* and *Akely*, near *Buckingham*. I was induced to do this *at that particular time*, because the Grand Junction Canal was in its progress to my immediate neighbourhood; which, like every other great work employing vast bodies of men from distant quarters, would probably introduce the small-pox. It was my wish that the labourers of these parishes should have the benefit of the high wages given on such occasions, without being exposed to the danger of that dreadful pestilence.

Having been in the habit of administering medicines to the poor, my offer to inoculate them was very generally accepted; and especially as most of these people are employed in milking. The common answer of such persons to my proposal was, "*We all know that nobody EVER DIED OF THE COW-POCK, and we all know that nobody EVER HAD THE SMALL-POX after it: But what an odd thing it is, that any body should think of INOCULATING with it!*"

For my part I thought it very odd, these two facts being so generally known, that no one should have attempted it sooner.

I had no intention of proceeding in this practice beyond my own parishes; but I was soon applied to by a clergyman, to whom I have been more than twenty years curate, to inoculate at *Greens-Norton* near *Towcester*, the small-pox having broke out in two families. I readily consented on condition that he would prepare the minds of the people, to whom I was but little known. In this he met with opposition, and in the result, about *five hundred* persons were inoculated with the *small-pox*, and *twenty-eight* by me, with the *Cow-pock*.

I started the same day as the hired inoculator. On the eighth I inspected the parties, and finding that they were all decidedly infected with the *Cow-Pock*, I desired them to give what

assistance they could to the people, who were falling very fast with the *Small-Pox*, and in great distress for nurses; *two hundred*, at one time, being in a helpless condition. Of these *twenty-eight* patients of mine, many slept with *Small-Pox* patients, and even with some, who died in a most dreadful condition.

The neighbouring villages were satisfied with this test, and in the following month I inoculated more than *one thousand* persons, who were apprehensive, that a very great fair at *Towcester* on old May-day, would spread the *Small-Pox* over the whole surrounding country.

On the application of clergymen and other respectable inhabitants, I have inoculated, within ten miles of my residence, upwards of *four thousand seven hundred* persons, many in situations greatly exposed to infection.

In Autumn, 1804, the *Small-Pox* raging among the people employed at the tunnel of the Grand Junction Canal, I inoculated in the neighbouring towns of *Stoke-Bruern*, *Shuttlehanger* and *Paulerspury*, *five hundred and seventy*. In the summer of 1805 I inoculated *two hundred and seventy* at *Potterspury*, the *Small-Pox* being at that time in two houses of the village.

In the whole of my practice I have inflexibly avoided accepting any fee or present, except in two instances, where I had no choice. I am therefore not to be treated otherwise than as an independent evidence. In that character I make the following declarations:—

1<sup>st</sup>. After a practice of more than six years no instance has occurred of any one inoculated by me being afterwards infected with the *Small-Pox*.

2dly. I never, during that period, have seen a single arm, that required surgical assistance, or any other dressing further than a little oil or milk and water.

3dly. I never knew an instance of a life being endangered or a taint left in the constitution by the Cow-Pock. On the contrary I can produce persons, who date a period of health, unknown before, from the turn of the Cow-Pock: the disease having apparently a tendency to cleanse the constitution.

If any candid person wishes to be more fully informed, let him devote a fortnight to the full investigation of this statement *on the spot*; I promise him the use of my lists, and recommendations to fit persons in every parish where I have set my foot: and this is the only method I propose of supporting the above assertions; as local benefit to my neighbours, and not public fame or emolument, has been the object of

J. T. A. REED,

*Curate of Leckhamstead and Akely.*

Nov. 8th, 1806.



*Justice impels the Editor to introduce the following extract, as an appendage to the communication, in the last volume of the Museum, on the subject of the adulteration of British Malt Liquors.*

### CRIMINAL INFORMATION.

Sir Vickary Gibbs applied for a rule to shew cause why a criminal information should not be filed against the proprietor of a daily newspaper, for the publication of a libel upon all the porter brewers of the metropolis. The intention of the paragraph was to represent that the porter drank in this city was compounded chiefly of strong narcotic poisons, exceedingly destructive of the health of the inhabitants. Although the

libel was of a very general nature, without attaching itself upon particular individuals, yet it was in such a form, that the court would not hesitate in granting the information, when they considered that it was capable of overthrowing the whole trade. It was stated in the libel, that the chief ingredients composing London porter, were *opium*, *solanum*, or night-shade; *datura stramonium*, or thorn-apple; *cynoglossum*, or hound's-tongue; *Coculus Indicus*, *nux vomica*, and *tobacco*; and the publication attributed most of the apoplexies in London to the use of this beverage; further, lest the *dictum* of the author should not be sufficiently satisfactory, it stated, that such was the opinion of the most learned physicians. Sir Vickary Gibbs added that his instructions were received from almost all the principal porter-brewers of the metropolis, who all deposed that their beer was never brewed with any of the ingredients charged in the libel, but solely with malt and hops. The learned counsel said he had likewise to make the same motion against five other newspapers.

The court desired that one of the affidavits containing the denial above stated, should be read, and likewise directed that an affidavit should be prepared, signed by the applicants, in which it should be stated, not only that the porter was brewed without containing any of the poisonous ingredients, but also that it was not sold when compounded with them. Until this was done, a rule *nisi* to shew cause was granted.

*English Newspaper.*

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## CORRESPONDENCE.

SIR,

I have been much engaged in business for some days past, which has obliged me to walk through our streets more frequently than I have been accustomed to, for many years; I



have been struck with the appearance of the mammillæ of our young females, they appear to be more prominent, from the age of ten to fifteen years, than they were in my youthful days. Pray sir, do you ascribe this, to a change in our *climate*, to the general *loose manners* of the age, or to the custom of reading *novels* and other *love-tales*? If my memory serves me, I think I have seen a remark made by an old physician, that the secretion of the seminal fluid in the male organs, is very much increased by reading lascivious books. May not the above works have a tendency to inflame the passions of our young girls, and by consent of parts, or as you physicians would say, by sympathy, operate on the lacteal vessels, and consequently prepare them *earlier* for that use, which nature has destined them to perform.

I wish some able physiologist would favor us through the medium of your useful publication with a solution of this question.

GAMMA.

P. S. Should the young ingenious men, who are in the habit of communicating to you their thoughts on generation, be fearful of offending by touching on these delicate parts, an essay in Latin or French will be acceptable to many of your readers, who are no physicians, but fond of prying into the arcana of nature.

*Philad. Pine Street, August 26th, 1806.*

JOHN R. COXE, M. D.

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#### DEATH.

John Vaughan, of Wilmington, Delaware.



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# MEDICAL AND PHILOSOPHICAL REGISTER.

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## FOREIGN AND DOMESTIC.

*Observations on the Camel's Stomach respecting the Water it contains, and the Reservoirs in which that Fluid is inclosed; with an Account of some peculiarities in the Urine.* By EVERARD HOME, Esq. F. R. S.\*

THE Board of Curators of the museum belonging to the Royal College of Surgeons, (late Mr. John Hunter's,) hearing that a camel in a dying state was to be sold, purchased it with a view of illustrating the anatomy of that animal; and Mr. Home, as professor of comparative anatomy, was appointed to examine the peculiarities of the stomach. The paper here published contains the report made on the subject.

The camel, the subject of the following observations, was a female, brought from Arabia; twenty-eight years old, and said to have been twenty years in England, and twelve years in the possession of the person from whom the Board of Curators purchased it. Its height was seven feet from the ground to the tip of the anterior hump.

In December, 1805, it came under the care of the committee. At that time it was so weak as hardly to be able to stand. It got up with difficulty, and almost immediately kneeled down

\* Philosophical Transactions. Part. II. for 1806.

again. By being kept warm, and well fed, it recovered so as to be able to walk, but was exceedingly infirm on its feet: and moved with a very slow pace. It drank regularly every second day six gallons of water, and occasionally seven and a half; but refused to drink in the intervening period. It took the water by large mouthfuls, and slowly, till it had done. The quantity of food it daily consumed was one peck of oats, one of chaff, and one-third of a truss of hay. Some of the urine was saved, and sent to Mr. Hatchett for the purpose of having it analysed: his account of its component parts is contained in a report annexed to this paper.

In the beginning of February, 1806, it began to shed its coat. Towards the end of March the wind became extremely cold, and the animal suffered so much from it, that it lost its strength, refused its food, and drank only a small quantity of water at a time.

In this state it was thought advisable to put an end to so miserable an existence: and it suggested itself to the committee that if this was done soon after the animal had drank a quantity of water, the real state of the stomach might be ascertained.

On the 1st of April, by giving the animal hay mixed with a little salt, it was induced to drink, at two different times in the course of two hours, three gallons of water; not having taken any the three preceding days, or shewn the least disposition to do so. Three hours after this, its head was fixed to a beam, so as to prevent the body from falling to the ground after it was dead, and in this situation it was pithed by Mr. Cline, junior, assisted by Mr. Brodie and Mr. Clift. This operation was performed with a narrow double-edged poniard passed in between the skull and first vertebra of the neck: in this way the medulla oblongata was divided, and the animal instantaneously deprived of sensibility. In the common mode of pithing cattle the medul-

la spinalis only is cut through, and the head remains alive, which renders it the most cruel mode of killing animals that could be invented.

The animal was kept suspended, that the viscera might remain in their natural state, and in two hours the cavities of the chest and abdomen were laid open, in the presence of all the members of the committee, and Mr. Chandler, a member of the Board of Curators.

From the comparative view here taken of the stomachs of the bullock and camel, it appears that in the bullock there are three stomachs formed for the preparation of the food, and one for its digestion. In the camel there is one stomach fitted to answer the purposes of two of the bullock; a second employed as a reservoir for water, having nothing to do with the preparation of the food; a third so small and simple in its structure, that it is not easy to ascertain its particular office. It cannot be compared to any of the preparatory stomachs of the bullock, as all of them have a cuticular lining, which this has not; we must therefore consider it as a cavity peculiar to ruminants without horns; and a fourth, or true digesting stomach.

“It is stated by authors that hares, rabbits, and even some men, ruminate; their doing so is not material to the present inquiry, since their stomachs, are not of that kind which makes rumination a necessary part of the process of digestion; and as far as I can learn from some persons who feed rabbits and fatten them with meal, although they have watched their rabbits with attention, they never saw them bring up the food into the mouth. It may therefore be only occasional when they eat particular kinds of vegetables. They have indeed a mode of working their lips when sitting quiet, which may have been mistaken for rumination. When it takes place in men it must be considered as a disease.

“ From the facts which have been stated, the following gradation of ruminating stomachs is established.

“ The ruminants with horns, as the bullock, sheep, &c. have two preparatory stomachs for the food previous to rumination, and one for the food to be received in after rumination before it is digested.

“ The ruminants without horns, as the camel, dromedary, and lama, have one preparatory stomach before rumination, and, properly speaking, none in which the cud can be afterwards retained before it goes into the digesting stomach.

“ Those animals who eat the same kind of food with the ruminants, yet do not ruminate, as the horse and ass, have only one stomach; but a portion of it is lined with cuticle, in which situation the food is first deposited, and by remaining there some time is rendered afterwards more easily digestible when received into the other, or digesting portion.

“ In comparing the teeth of those animals that ruminate, with those of the horse and ass, which live on nearly the same kind of food, the following peculiarities are met with.

“ The ruminants with horns have molares in both jaws, and incisores only in the lower jaw.

“ The ruminants without horns have, in addition to these, what may be called fighting teeth, or a substitute for horns. These are tusks in both jaws, intermediate teeth between the molares and tusks, and in the upper jaw two small teeth anterior to the tusks; none of which can be of any use in eating.

“ The camelo-pardalis forms an intermediate link in these respects. It has short horns, and has no tusks.

“ The molares in both these genera of ruminants are open in the structure of their crown, which is not horizontal but oblique ; the outer edge in the upper jaw and the inner in the lower jaw being the most prominent, so as to adapt them to each other. The lower jaw has less width than the upper, so that the lower molares fall considerably within the upper : when the animal eats, it can only masticate with one side of the mouth at a time, by bringing the lower jaw to that side, so as to make the teeth of both jaws oppose each other : the teeth of that side are applied to the food three or four times, and then those of the opposite side.

“ This mode of mastication appears to be peculiar to the ruminants, and is certainly very different, and much more imperfect, than the mastication of the horse, whose molares are very compact in the texture of their crowns, and are opposed directly to each other by horizontal planes.”

The urine of the camel was found to contain, in 100 parts, Phosphate of lime, 3—Muriate of lime and of ammonia, 15—Sulphate of potash, 6—Carbonate of potash and of ammonia, 4—Urea, 4—Water, 65. It is remarkable that the uric acid should be found in the camel's urine, as it is the first instance on record, as far as relates to the urine of graminivorous animals.

It appeared from other experiments, that potash is the only fixed alkali present in the urine of the camel, cow, guinea-pig, and rabbit. In this respect, the urine of the horse is peculiarly distinguished from that of those animals, as it is found to contain abundance of soda. The urine of both the horse and the ass changes vegetable blues to green, but is destitute of ammonia.

*Med. and Chir. Rev.*

*A Case of Inflammation extending through the Circulating System.\**

‘ Sarah Andrews, a lusty young woman, about twenty-three years of age, who was very subject to hysteric fits, had just recovered from a slight fever ; when, without any external cause, that was apparent, she was attacked with pain which extended all over her body, and gradually increased to a great degree of violence. It was attended with a hot dry skin, great prostration of strength and spirits, and an inattention to external objects. She laid moaning and crying out with pain, and very unwillingly answered any questions that were put to her, but her answers were perfectly rational.

‘ Her pulse was a very peculiar one : I never felt such a pulse before nor since. I hardly know how to describe it : it was extremely hard, quick, unequal, and irregular, having at the same time a peculiar tremulus. On applying my hand to the region of the heart, I found its action exactly correspond to the pulse. I took away sixteen ounces of blood in different cups. The first portions gave a very thick buff upon the coagulum, which was very firm and cupped. The pulse was lowered and became softer, and the patient faint, but the peculiar tremulus continued.

‘ This woman was a patient to my friend Mr. Slater, of Margate ; a week passed before I had an opportunity of seeing her again. At this time the universal pain had greatly abated, the patient was extremely low, the extremities, both hands and feet, were cold and œdematous. The pulse was very low, but still it retained the same irregular trembling action, and so did the heart. Her respiration was quick and short ; an horizontal posture was extremely disagreeable to her, although it did not, as she said, increase the difficulty of her respirations. From

\* Weldon’s cases and observations in Surgery.

this time she gradually sunk, the œdema became general, and she died in about three weeks.

‘ On dissection I found all the viscera of the thorax and abdomen in a healthy state, no adhesion of the lungs to the pleura, no fluid in the cavities of the pleura, nor of the abdomen, and very little in the cavity of the pericardium. The heart had undergone no change in its structure, nor yet its valves. But the internal surface of both ventricles and auricles of the heart was of a florid red colour, evidently produced by inflammation, with small irregular patches of coagulating lymph adhering to it.

‘ In the aorta these appearances were more remarkable, from being contrasted with its natural colour. The small vessels on its inner surface were so completely injected with red blood, from preceding inflammation, as to give the whole surface an uniformly florid red colour: small patches of coagulating lymph had been thrown out by the vasa vasorum, and were adhering to the surface in the form of a thin delicate membrane, as in the heart.

‘ I examined one of the carotids, the internal and external iliacs, the popliteal artery, and one of the axillary arteries: they were all in the same inflamed state. The inner surface of the pulmonary arteries and veins, of the vena cava, of the axillary and popliteal veins also, were inflamed, although in a less degree; but there was no effusion of coagulating lymph adhering to them. The head was not opened.

‘ The body had no appearance of wound in any part of it. The wound, from bleeding, had healed in a few days.’

*Med. and Chir. Rev.*

*Great habitual Use of Corrosive Sublimate.*

There is said to be a man now living in Constantinople, who has attained the great age of 106, and who has been in the habit, for thirty years past, of taking daily 60 grains of corrosive sublimate. When young, he was, like the Turks in general, addicted to the use of opium; but having taken by degrees a large quantity without the desired exhilarating effects, he substituted the corrosive sublimate, as a stimulant, in its stead. Lord Elgin (the English ambassador,) Mr. Spencer Smith, and many others now in England, have met this extraordinary person, and have heard him say that the sensation he experienced from the sublimate was the most delicious he had ever enjoyed.

*Med. and Chir. Rev.*

*Caution respecting Angustura Bark.*

The Senate of Hamburgh has published a caution against a kind of *Angustura* bark which has been sold in that city, and operates as a poison. It came from Spain as the true bark, of which it has some external resemblance. They may be distinguished, however, by the following characters: the decoction of the true *Angustura* bark dyes linen yellow, is not turbid, nor is altered by a solution of iron; that of the spurious kind does not dye linen, and becomes black with chalybeates.

*Med. and Chir. Rev.*



*Kinglake's Remarks on Angustura Bark, addressed to the Editor of the Med. and Chir. Review.*

SIR,

In the last number of your publication appeared an article relative to a poisonous species of *angustura bark*. If what has recently occurred in this town from the use of a drug sold under the appellation of *angustura bark* should also have happened in other places, the evil of having such a pernicious substitute on sale is of a most threatening nature, and ought, without delay, to be exposed and obviated.

In five instances within these three years, a drug sold in this town for *angustura bark*, and administered as such to four different persons, evidently produced the most distressing effects. In four of the instances alluded to, an approach to syncope, accompanied with more or less of universal tremor and spasmodic twitchings, are said to have occurred. In the fifth instance referred to, it was exhibited in a case of low intermittent fever, and was speedily followed by effects similar to those which presented in the other instances. On this occasion, however, death ensued; but whether that event be justly attributable to the influence of the medicine or to that of the disease, is doubtful. In each of the other instances, the patients suffered severely from the ailment induced, during some hours, and its hurtful effects endured several days.

In the article of intelligence recorded in your publication, respecting the true and spurious sorts of *angustura bark*, it is stated that "they may be distinguished by the following characters: the decoction of the true *angustura bark* dyes linen yellow, is not turbid, nor is altered by a solution of iron; that of the spurious kind does not dye linen, and becomes black with chalybeates." *Med. and Chir. Review*, No. 83.

With a view to ascertain whether the quality of the *angustura bark* sold in the shops of this town could be detected by the respective tests here cited, I procured a specimen of the drug from three different druggists residing in this place. A separate decoction was made of the several parcels, and I regret to add, that each of them clearly denoted what is affirmed to be the distinguishing characters of the *spurious* and *poisonous* sort of the drug. To pieces of linen cloth infused in each decoction, was imparted a dusky hue, in which the yellow colour, said to be characteristic of the true species, was scarcely perceptible; each decoction was also both turbid and deeply blackened by the admixture of a solution of iron.

If these are unequivocal tests of the noxious kind of *angustura bark*, it surely becomes incumbent on the college of physicians, or on some other adequate authority, immediately to institute such an investigation of the subject as may hereafter exempt mankind from the deleterious effects of a *spurious* drug, which, in the full confidence of its being *genuine*, is very commonly directed as a mild and salutary tonic.

The inquiry, indeed, appears to be additionally necessary, in as far as *angustura bark* is usually resorted to in cases of chronic weakness, from various causes, and more particularly in the convalescent periods of febrile affection. When exhibited under these circumstances, it is difficult correctly to mark its effects. The symptoms of the disease it is then directed to remedy, so much resemble those of its own hurtful operation, that any aggravation of complaint occurring during its employ, would be naturally imputed rather to an unfavourable course of the disorder, than to any conceivable injury from the medicine. Under this ambiguity of effect, therefore, it is not extraordinary that the injurious agency of a spurious sort of *angustura bark* should have escaped general notice.

Further research into the subject will probably furnish additional criteria by which the true sort of *angustura bark* may

be distinguished from that which is spurious with more promptness and certainty than can at present be done. It must be allowed to be an object of no small concern to the public welfare, to have a drug like *angustura bark*, high in both medical and popular estimation, liable to be counterfeited by an article, said to "operate as a poison," and which, in the several instances here adduced, did actually produce effects truly alarming.

I am, &c.

ROBERT KINGLAKE.

Taunton, February 13th, 1807.

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*Tape Worm.*

The Russian Archiater, *Von Beck*, has published the following as a remedy for the tape-worm: *R. merc. dulc.* ℥j—*corn. cerv. ust—cinnabar. antimon.* ana gr. x. m.—This is to be taken at 4 o'clock in the afternoon, and at supper some broth, with 2 oz. of oil of almonds. Two hours after, it is said, the tape-worm comes away: but in case this should not happen, a drastic purgative is given the following morning, and repeated, after two hours, if the whole worm should not be brought away. This purgative consists of a scruple of jalap and an equal quantity of the leaves of the *felix mas*, with ten grains of gamboge.

*Med. and Chir. Rev.*

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*New Discoveries in Galvo-Electricity.*

1. About fifteen years ago, Messrs. *Van Troostwyk*, *Dieman*, and others in Holland, produced gas by passing the electric

spark through water in tubes sealed at one end ; which gas was concluded to be the constituents of water, viz. hydrogen and oxygen gas—because, in passing the electric spark through it, between two metallic points, over water, it again disappeared. In this country, no one could succeed, on repeating this experiment, till Mr. Cuthbertson arrived from Holland, where he had resided many years, and had performed many experiments with his own capital instruments, in the *Teylerian Museum* ; and particularly the curious experiment, in which water was supposed to be both decomposed and re-composed by electricity.

2. About the year 1794, Dr. Pearson's paper was published in the *Philosophical Transactions*, containing experiments, in which he showed how the experiment of the Dutch chemists was to be performed, and demonstrated that the gas above-mentioned was really a mixture of hydrogen and oxygen, as had been before supposed ; for he collected it, so as produce the explosion over mercury ; thereby manifesting the production of water by composition. But it was supposed that powerful electrical machines were requisite for effecting the decomposition of water by the electrical spark.

3. Dr. Wollaston, however, shewed that such powerful machines were not necessary, provided the wires, over which the electrical fluid passed, were of sufficient tenuity ; in which case almost a pocket-machine would answer the purpose.

4. A new kind of electrical apparatus was invented by *Volta*, viz. by the alternate disposition of plates of two kinds of metal, with water and saline matter interposed. This contrivance is well known at present by the name of the *Voltaic Pile*. With this new instrument, the recently-discovered galvanic or galvo-electric fluid was extricated, and many wonderfully curious experiments were executed by different philosophers, both in this country and abroad. It was observed

to be extricated by other heterogeneous substances applied to one another, besides metals, and manifested itself by the most delicate tests, viz. living parts of animals, and even, as was said, the blood itself, and living vegetable matter. From these experiments, it was concluded that either a peculiar state of the electric fluid, or a newly-discovered fluid analogous to the electric, was perpetually passing from one body to another when in contact, owing to its being in unequal quantities in them, or in order to restore an equilibrium; being, as it is termed, in the positive or *plus* state in one, and in the negative or *minus* state in the other.

5. Mr. Carlisle and Mr. Nicholson discovered that, during the extrication of the galvo-electric fluid by the pile of Volta or trough of Cruickshank, water was decomposed when a wire of one metal communicated with one extremity of a given metal, and another wire with the other extremity of another given metal: but the most curious part of the process was, that the hydrogen gas was always extricated at the silver end, and the oxygen at the zinc end. Hence these were called the two poles: the silver, for example, was the *negative*, and the zinc the *positive* pole. This property is altogether inexplicable on the principles of common electricity; and seems alone to establish galvanism as quite a different species from electricity.

It became now a question,—whether galvanism was the effect of the oxidation of metals, or merely manifested itself in passing from one to the other; and that, in so doing, if water was in its way, a decomposition was effected; and if an oxidable metal, as zinc, was present, the oxygen united to it, and the hydrogen was disengaged at the opposite extremity by the other metal.

6. In operating upon water with galvo-electricity, Mr. Peele\* of Cambridge announced, that he could procure soda, lime, muriatic acid &c. from water purified by distillation; and Pacchioni, in Italy, reported, that he could produce, by the same agent, muriatic acid from pure water, by disoxygenation. These asserted facts were contradicted by the experiments of some, and obtained no credit with others. The questionable authority of Peele was indeed such, that many considered his experiments to be mere forgeries; and the editor of the journal who published them was looked upon as having been a dupe to his credulity. Pacchioni's experiments, in the mean time, received support from some chemists, although discredited by others.

7. Mr. Davis seems at once to have unravelled the mystery of the pretended Mr. Peele's experiments, and confirmed those of Pacchioni. At the Bakerian Lecture of the Royal Society, three months ago, Mr. Davy demonstrated, by a set of most ingenious experiments, the agencies of galvo-electricity on bodies chemically united. He shewed its power in disuniting them, so as to manifest new substances where least of all suspected; and in decomposing certain compound bodies known to be present, such as double salts with earthy, metallic, and alkaline bases. When such substances are decomposed in a *Voltaic Circuit*, in water, the acids arrange themselves around the *positively*-galvo electrified metallic point, and the bases around the *negative* point. In this way, even insoluble bodies are decomposed; as, various stones and minerals. What is most wonderful, acids and alkalies, by the attractive and repulsive agencies of the galvanic fluid, are carried through and out of menstrua for which they have the strongest chemical attraction, and through both animal and vegetable substances.

\* We believe this to be an assumed name, and that the real author is unknown to this day.

Mr. Davy accounts for the separation of acids from alkalies, earths, &c. by supposing electricity to exist in opposite states in acids and alkalies, as well as in zinc and copper. Thus negative and positive galvo-electric energies are exerted, and bodies naturally *negative* are repelled by negative electricity, and bodies naturally *positive* are attracted by negatively electrified points. Finding that chemical attraction is destroyed, increased, and diminished, according to the galvo-electrical states of bodies; and as all bodies which unite chemically seem to be in opposite states of electrical energy, the question is proposed,—Whether chemical unions and disunions are not occasioned by the varying states of these energies; or whether chemical attraction is not the same thing as electrical energy?

As acid and alkali, lime, &c. were got by the *Voltaic Circuit* from water supposed to be pure by distillation, Mr. Davy imagines that they might proceed from the decomposition of substances in the water between the plates of the pile, and be transported in the circuit: also, that water is not rendered perfectly pure by distillation. In short, according to these experiments, the attracting and repelling agencies of galvo-electricity act not only upon water, as discovered in the first instance, but upon all other bodies in nature.

Hence two highly important principles seem to be discovered, viz.

1. A theoretical one,—that chemical attraction depends upon galvo-electricity.
2. A practical conclusion, which is, a new power of analysis.

These experiments of Mr. Davy, and the late work of Chemical Statics by Berthollet, probably exhaust the subject of



chemical attraction, first proposed by Sir Isaac Newton, and afterwards so successfully cultivated by Boerhaave, Stahl, Freind, but above all by Cullen.

It will now remain for physiologists to discern the agencies of the galvo-electrical fluid on living matter;—an entirely new field of observation, if we accept the little already done by *Galvani, Valli, Volta, and Aldini.* *Med. and Chir. Rev.*



### *On the Composition of the Muriatic Acid.*

The experiment of M. *Pacchioni* relative to the composition of the muriatic acid and soda by the action of the galvanic pile, has been unsuccessfully tried in this country and in France; nevertheless M. *Buch* observes that the experiment always succeeds when made in the following manner.

Erect two galvanic piles near together, and connected with one another at the bottom, in such a manner that the two poles, the one *positive*, and the other *negative*, shall be at the superior extremities of the two piles. Conduct a gold or platina wire from the negative pole (zinc) into a tube (A) lightly closed at top by a cork or piece of linen, which the wire traverses, descending about two-thirds of the tube. Another wire of the same substance is to be carried from the positive pole (copper) into another tube (B) similarly disposed. Both tubes are then to be plunged in a glass tumbler containing distilled water. Oxygen then is found to be disengaged in the tube B, and hydrogen in the tube A. After a few hours, unequivocal marks of the existence of the muriatic acid takes place in the tube B, and of soda in the tube A. In the space of ten or twelve hours, the nitrate of silver, being dropped into the water, becomes precipitated in abundance. If



the water of the two tubes is mixed together and evaporated, there is obtained a notable quantity of muriate of soda. If the experiment is made in the dark, the acid found in the tube B is oxygenated, as is easily ascertained by its peculiar odour and colour. This oxygenated acid soon attacks the gold wire, and the *powder of Cassius* is obtained on evaporation.—The discovery of the formation of soda is due to *Mascagni* of Siena.



*Extract of a letter from Mr. W. H. PEPYS, junior, of London, to Dr. JAMES WOODHOUSE, Professor of Chemistry in the University of Pennsylvania.*

We have been extremely interested lately, with some galvanic experiments, made by Mr. Humphrey Davy.

The negative or the positive ends of the trough of Cruickshank, have the power of completely decomposing all chemical compounds, solid or fluid.

The method of making those on solids, is by drilling two holes, in two pieces of sulphate of lime or plaister of Paris. For instance, they are placed upright, filled with distilled water, and the positive gold wire is put in one, and the negative gold wire in the other: a syphon or communication is then made between the two, by a piece of fibrous gypsum or asbestos.

In a few minutes by the test papers, an acid is found in one, and an alkali in the other. The experiment being continued, gives sulphuric acid in one, and a solution of lime in the other. The acids arranging themselves on the positive side, while the alkalies and metallic oxides go to the negative.

Metallic wire not oxidable, and pure distilled water should be used, to have the effect.

Some of the decompositions are attended with deflagrations, as the concentrated nitrate of ammoniac. Gold cones or cups, containing about eight or ten drops of solutions, with an asbestos syphon, are extremely useful for these experiments.



*The following interesting Galvanic Experiments, were made by  
Dr. JAMES WOODHOUSE, Professor of Chemistry in the University of Pennsylvania.*

The doctor having placed a quantity of mercury in a plate, covered it to the depth of an inch, with distilled water. He then introduced an iron wire, connected with the copper pole, of an apparatus, formed of sixty plates of copper and zinc, four inches square, into the mercury, and immersed the other wire, applied to the zinc pole, into the water, so as to bring it as nearly as possible in contact with the mercury, without touching it.

Immediately a constant stream of vivid and intense light, issued from the end of the wire, which could be kept up any length of time.

It was accompanied with a hissing noise, and an oxidation of the iron.

The light was produced from wires of platina, gold, silver, copper, zinc and tin, and from the zinc and copper poles, and was visible in spermaceti oil, oil of turpentine, spirit of wine,

fulphuric acid, carbonic acid gas, azotic air, nitrous gas and pure inflammable air, when placed over mercury.

It was not greater in oxygen air, than in carbonic acid gas, and was of the colour of the electric light.

When a piece of fine iron wire, half an inch in length, was laid upon the mercury, covered with water, and the copper pole wire was immersed in the mercury, and the zinc pole wire was introduced into the water over the wire, it was repelled with great velocity, and the whole of the mercury was violently agitated, and when any light substances, were found swimming on its surface, they were dispersed in all directions.

By means of gold wires, placed in a solution of pure caustic potash, or of the pearl ash of the shops, the Doctor obtained five cubic inches of oxygen and hydrogen gas, of a high degree of purity, in fifteen minutes; whereas pump water, tried under the same circumstances, for the same time, yielded but a fourth of a cubic inch of these airs, contaminated with forty per cent. azotic gas.

The agent Dr. Woodhouse uses, to excite the galvanic influence, which has never been tried in Europe, is a solution of the sulphate of copper or blue vitriol. It acts in the same manner as the nitric or sulphuric acids, by giving oxygen to the zinc, but is preferable to them, as it does not produce either nitrous air or hydrogen gas.

He considers the galvanic influence, as depending altogether upon oxygen, without which it cannot be produced.

*On the Use of Tannin in Medicine.*

M. Pezzoni, a physician residing at Constantinople, observes in a letter to M. Careno, that he has found the substance called *Tannin* to be productive of the greatest service in a variety of complaints depending on relaxation and debility, and to be superior in this respect to the Peruvian bark itself. He has employed it both internally and externally, and ascribes the good effects to its chemical properties, and its peculiar affinity with animal matters. He has always administered it internally in the form of pills, sometimes alone, sometimes in combination with other substances, as opium, camphor, musk, digitalis, iron, emetic tartar, &c. according to the particular circumstances of the case. The greatest quantity he has yet ventured to give is from eighty to a hundred grains, in divided doses, in the space of twenty-four hours. He applies it externally in cases of gangrene, dissolved in alcohol, and sometimes in simple water.

*Med. and Chir. Rev.*

*Process for separating Quicksilver from Animal Fat.*

The volatility of mercury makes it rise in distillation with animal fat, with which it has been intimately blended; and when the mixture undergoes combustion, the mercury is dissipated in vapour: it is difficult, therefore, to obtain the mercury from this composition, or to ascertain the exact state in which it enters into combination with the lard. M. Mathieu, professor of physic and philosophy, was enabled to succeed in this point, by dissolving the lard in a boiling solution of potash; when the mercury was precipitated in the form of a greyish paste. This matter being washed and thrown on a filtering paper, was quickly reduced to its metallic state by

mere friction. In this way, M. *Mathieu* recovered the whole of the mercury contained in the ointment.

As mercury produces its specific effects more powerfully, when in combination with animal fat; and as in this form it is but little removed from the metallic state, M. *Mathieu* conceives that the acids with which it is usually combined add nothing to its virtues, but that it really acts in the metallic form: the combination with fatty matters, he thinks, only answers the purpose of applying it more easily, and in a state of extreme division: finally, that it is *reduced* by the friction employed in its application, and enters the system in its metallic form, and not in the state of an oxide, as commonly supposed.

*Med. and Chir. Review.*



*Of the Nature of the Air derivable from Water.*

Water contains air, as air contains water. *Humboldt* and *Gay-Lussac* found, that the air expelled from water by boiling, contains more oxygen than atmospheric air, but that the quantity varies at different periods of the process.

Water, on being heated, gave out at first	
air which contained of oxygen	23 per cent.
At a short period afterwards	- 27
At a third period	- 30
And at the moment of ebullition	- 32

The air furnished by snow contains as much as 34 per cent of oxygen.

*Med. and Chir. Rev.*

*On the Absorption of Azote in the Respiration of Vegetables.*

Vegetables, as well as animals, absorb azote in the act of inspiration, and exhale it in respiration, as appears from the following experiment.

One hundred parts of the air expired by plants, mixed with three hundred parts of nitrous gas, left a residue of from one hundred and ten to one hundred and twenty; which proves that the expired air contained a large quantity of pure air or oxygen, together with a small portion of azote. This azote combines itself equally with, and enters into the composition of, the gluten and other products of vegetables which contain a large quantity of azote.

*Med. and Chir. Rev.*

*On the Virtues of Oxygenated Water.*

M. Odier, of Geneva, observes, that he has been long in the habit of prescribing this water with good effect in cases of hysteric spasms and cramps, particularly of the stomach and intestines. He has found it equally serviceable in affections of the chest, that were of an asthmatic, rather than a phthical nature. It is also useful, he says, as a tonic, in cases of debility following fevers, and as a diuretic in anasarca and other species of dropsy.

The oxygenated water has neither taste nor smell; the oxygen is not combined with it by the intermedium of any base, but simply by the force of compression: hence it is necessary to swallow it quickly when poured out of the bottle, and to keep it very closely corked. Water can be made to absorb in this way about half its bulk of oxygen gas. M. Odier

remarks, that, when the oxygenated water produces dysenteric symptoms, this effect may be removed by water similarly impregnated with hydrogen. *Med. and Chir. Rev.*



### *On Tinned Utensils for Domestic Purposes.*

A great deal of prejudice exists with regard to the adulteration of tin coatings, to copper vessels, by the admixture of more or less of lead with the tin, from motives of cheapness. A number of experiments have been lately made by the celebrated Spanish chemist M. *Proust*, which shew that there is little or no foundation for the fears which are commonly entertained on the subject. These experiments lead to the following conclusions.

1. Confectioners may continue to use untinned copper vessels, provided they adhere strictly to the rules of their art with regard to cleanliness.

2. Every measure which tends to oblige them to employ tinned vessels will turn out to be illusory; as the heat necessary for the greater part of their compositions destroys the tinning in a few days. M. Proust caused different sweetmeats to be prepared under his own eye, in vessels coated with fine tin; but the workman could not succeed. Some of the preparations were burnt: the tin was detached by the wooden spatula employed in stirring the mixture, and the copper laid bare.

3. The tinning that is adulterated with equal parts of lead is not attended with any danger, as lead alloyed with tin is neither soluble in lemon juice nor in vinegar,—the two acids



the activity of which is most to be feared: the tin, in this case, being more oxydable than lead, becomes dissolved exclusively in these acids, and prevents the latter from being attacked; while the lead cannot appropriate to itself an atom of oxygen, without being deprived of it at the same instant by the tin.

4. Lead, alloyed with equal parts of tin, never becomes oxydated and dissolved before the latter. The same alloy taken internally, and in a much larger dose than could be swallowed by a whole family in any case, even supposing that all the tinned vessels in the house should not last for eight days, is not of the least detriment to the health. No disadvantage, therefore, can arise from the usual practice of workmen in alloying their tin with one third, or a quarter, of lead. And the same is true of cups and other vessels made chiefly of tin or pewter, in which there may be a mixture of lead.

In consequence of this report, made to government by M. Proust, two commissioners were appointed to inquire whether acid liquors kept, for a longer or shorter time, in tin vessels with the usual alloy of lead, really became impregnated with any portion of the latter, so as to threaten to be injurious to health. They chose for the purpose, out of more than a hundred vessels that had been seized and condemned by the police, such as appeared to have the largest quantity of lead in their composition, and which were the most corroded by moisture. They suffered acid liquors of all kinds to remain in them for four days together, and then examined the liquors with the greatest precaution. The result of their experiments was, that neither vinegar, verjuice, oranges, cherries, gooseberries, sour milk, &c. became impregnated with any particle of lead, discoverable by the tests of sulphate of potash, hydro-sulphuretted water, liver of sulphur, or any



other test; but most of them took up a small portion of tin. The consequence of these trials was, that the seized vessels were restored to their owners. *Med. and Chir. Rev.*



*Observations on the Elephantiasis\*.*

“*Chap. 18* is a paper on the *Arabian* leprosy, or *Elephantiasis* of the ancients,—the *elephantiasis* of the moderns, or the *Barbadoes leg*,—and the *Lepra Grecorum*. It was drawn up, we are informed, during the author’s residence in the island of *Madeira*, and was intended as a supplement to *Dr. Thomas Heberden’s* communication on the same subject, published in the *medical transactions* of the *London college*. It is particularly valuable, in pointing out the distinction between diseases that have some symptoms in common, but which have been confused by having the same name given to them.

The *Arabian leprosy*, or *elephantiasis* of the ancients (the disease here particularly described,) is principally characterized by the following symptoms: tubercles arise about the face, particularly on the external ear, *ala nasi*, eye-brows or forehead; these tubercles are of a higher colour than the sound skin, and are for some time smooth and glossy. After a time, they crack, and become covered with a white furfuraceous substance. As the disease advances, the features become horridly distorted; whence probably the epithets *satyriasis* and *leontiasis*, applied by writers to these wretched objects. The inside of the mouth is also rendered irregular and thickened, and the bones of the nose sometimes give way. Similar tubercles, though less elevated, are observed on the limbs, and also occasionally on the body, though to a less extent. The legs are in most cases covered with the furfuraceous substance,

\* *Adams’ observations on morbid poisons.*

or with foul but superficial ulcers. In the upper and anterior part of the thigh there is, in almost every case, a firm glandular-like swelling, indolent, and without any disposition to suppurate. The feet are often so affected with foul ulcer that the toes drop off, one after another. The fingers are sometimes so contracted as to lose all power of motion. In a few instances there is a loss of sight. It appears that the disease is hereditary, or rather the disposition to it; but the children of leprous parents are by no means necessarily affected with the disease: and in many it appears to arise spontaneously without such an origin. It is a curious circumstance attending it, that those who are attacked before the age of puberty, never arrive at that state, and that, generally speaking, others lose the disposition to procreation: the beard falls off, and the organs of generation waste.—Corresponding circumstances occur in the female. There seems no reason to suppose the disease to be infectious; and it is believed to be incurable. Yet though the persons affected rarely arrive at old age, they, for the most part, seem to die of other complaints.”

*Med. and Chir. Rev.*



*Of the Itch Insect.\**

“*Chap. 19* contains ‘An account of the *Acarus Firo* (*exulcerans* of Linnæus), by some considered as the itch insect,” read before the Royal Society, April 1805.—It has been much disputed among naturalists and physicians, whether or not the itch depended on the presence of animalculæ; and even many of those who have admitted the existence of animalculæ, have denied that they were the cause of the disease, and supposed that the vesicles merely formed a convenient nidus for their ova. From the account given by Dr. Adams in the paper be-

\* From the same.

fore us, it seems probable that two very distinct affections have been confounded together, one of which really depends upon the presence of animalculæ, while the other (the common itch) is of a different origin.

The former is frequently met with in Madeira, and the name given to it is ouçao or ouçam, the last syllable being pronounced like the French nasal terminations. When the disease reaches the head, so as to produce a general scabbiness, it is called zagra. This affection evidently depends upon an exceedingly minute insect, not unlike the *acarus farine*, but considerably smaller. It occasions small vesicles resembling those of the itch, with which it is easily confounded. It differs however from the itch, in having extended from the vesicle a slender red line, about a quarter of an inch in length, at the extremity of which usually the insect is found, under a reddish elevation (the insect is never found in the vesicle itself). It differs from the itch, also, in being attended with considerable general irritation or fever. It appears to be curable by the usual remedies for the itch, viz. sulphur and the mercurial applications; but with more facility than the itch, though it is ready to return if the remedy is not occasionally resorted to for near a month, during which it is probable that young insects are evolved from their eggs, or arrive at such a state of existence as to propagate.

The author, and his friend Mr. Banger, were curious enough to get themselves *infected* with the ouçao, in the month of July, by the help of an old woman, who displayed much discernment and dexterity in detecting the insect, and applying it to the skin between the fingers. "For more than three weeks," says the author, "no inconvenience was felt. From that time began frequent itching in different parts of the body and arms; but no eruption could be discovered. In less than a fortnight afterwards, my arms and belly were covered with a general efflorescence, yet but few vesicles appeared. I ap-

plied to my old woman, who readily drew two ouçoes from my arm, but not from the vesicles ; indeed on this, and on all other occasions, I could not help remarking that though I could not discover what the marks were by which she was directed, yet she constantly passed over the vesicles, without suspecting them to be the nidus of the insect. At length I perceived it was a small slightly-discoloured elevation of the cuticle, which appeared as if soon likely to become vesicular, that the woman always attacked, but not always with success. However, she always constantly answered to our inquiries, that where the bladder was formed, the ouçao had left the spot.

“ No remedies being used, nor any alteration in diet or my usual habits, the weather also being warmer than common, even for this season of the year, the disease continued to spread rapidly, so that, by the end of August, my whole body, arms, and thighs, were covered with the efflorescence. As, however, the vesicles were few, I was willing to believe the eruption might be the prickly heat. On this subject I applied to my old woman, who confirmed my suspicions: but there is reason to fear I was not sufficiently cautious in leaving my teacher to form her own opinion before she had discovered mine. In the mean time my health suffered exceedingly, not only from the inconvenience produced by the itching; but about noon a quotidian fever began, with a slight shivering, and was succeeded by headach, dry heat, great thirst, loss of appetite, and considerable exacerbation of the itching. The consequent perspiration was not greater than what the season might have produced.

“ So little was I prepared to expect such effects to arise from such causes, or so determined to ascertain the cause, that, to allay the itching, I used an ointment of pomatum and saccharum saturni, from which some relief seemed to follow. It was, however, for a short time, and, the paroxysm of fever

being as regular though much slighter than in common ague, I had recourse to the bark. If this produced relief, it was very temporary.

“ By the middle of October the efflorescence was universal over my abdomen, and very general over my arms, breast, and thighs. My hands were only slightly affected, but sufficiently to be detected by the natives. The character was, indeed, here more strongly marked, for the white shining cuticular elevations were such as I should not, in England, have scrupled to call the itch.

“ About this time one of my family became slightly infected with the eruption, but her fever was very considerable. It seemed therefore reasonable to attribute all my own symptoms to ouções, or at least it was time to try whether by ridding the skin of them the other symptoms would cease. The ointment I had invariably found successful in all other cases was composed of a drachm of white precipitate of mercury and an ounce of soft pomatum. The effect of this on us both was almost incredible. In three days time the itching nearly ceased, and the fever entirely. However, it was found necessary to have recourse to the ointment occasionally for near a month afterwards: little cuticular elevations and some vesicles arose at different times during that period; and, when they arose, were constantly attended with symptoms of fever. From that time we felt no further occasion for any remedies.”  
—The insect, it seems, has a power of leaping equal to that of a flea; which accounts for the quick spreading of the disorder.

*Med. and Chir. Rev.*

*Cow-pox, supposed variety of Small-pox\*.*

‘ The author expresses his conviction that the cow-pox is in reality a variety of small-pox. This opinion is supported by the following train of reasoning, with which we shall terminate our analysis of this valuable work.

“ I should conceive it lost time to offer even a summary of the arguments adduced to prove that cow-pox is a security against small-pox. There is, in my opinion, no medical fact that stands on a securer foundation. The very exceptions which have been made are so few, as to establish the law. In all inquiries we are to consider the credibility of witnesses. On the side of cow-pox they are not only the most numerous, but include those names to which the public looks up with the greatest confidence. On the other side, there are certainly respectable names, but their number is comparatively small. The publications, however, on the subject are so numerous, and have been so lately digested by Dr. Willan, that I shall leave the mere question of evidence, and content myself with offering a few proofs that the cow-pox and small-pox are the same morbid poison. These proofs shall rest on those laws which were established in the early part of this work.

“ The varieties of small-pox have been marked by different writers. There is one, however, which is unnoticed by all, not excepting Sydenham: probably he had only seen it so casually as not to think it deserving of particular notice. This variety I wish to call the *pearl* sort, because the nearer the pustule arrives to the figure and colour of a pearl, the more perfect is the character of the disease.

From the same; last chapter.

“ Dr. Jenner in his first ‘ Enquiry into the Nature of Cow-pox,’ mentions a small-pox, which for some time spread through the county of Gloucester so mild, that the lower orders of people lost all their terrors of it, and the usual intercourse was maintained, as if no such contagion existed. No other description is given than that it never became confluent, and that it was, in a given number of subjects, as mild as if they had been all inoculated\*.

“ The most striking distinction in the disease to which I refer, is, that, contrary to what Sydenham observes both of the distinct and confluent, the pustules in the face remain white, like those on the body, till they scab. In the best marked cases the pustules are never very large, but particularly round. As they increase, the upper surface extends over the base; and as they dry, the scab becomes nearly globular, that is, the whole is distinguishable above the skin, without concealing more of the sphere than what would happen were such a figure actually placed upon the surface. If the pustule is dissected, the slough at the bottom is found particularly thin. Hence, though the inflammation is inconsiderable, yet, the lymph being less diluted with pus, the scab is much harder, as well as more regularly formed, than from the common distinct small-pox.

“ This small-pox is not only thus regular in its appearance, but is uniformly mild, and is never attended with what Sydenham calls the secondary fever, that is, the symptomatic fever from skinning. Thus, then, there may be a variety in small-pox which proves permanent under inoculation.

\* “ Enquiry,’ &c. First Part. I cannot now tell in what Journal or Transactions I have read an account of a small-pox so generally mild, that after a time no one took the trouble to inoculate, but exposed themselves without fear. A.



“The next thing I would remark is, that small-pox and cow-pox, contrary to the law of all morbid poisons, which are different in their nature, will proceed together in the same person, without the smallest interruption of each other’s course. If inserted nearly at the same time, in the same person, each proceeds in the same course as if they were in two different subjects :—if inserted nearly in the same spot, the two form one common areola; but the vesications are distinct, and each preserves its own character, till that of small-pox becomes purulent from suppuration for the separation of the slough. If secondary pustules follow from the small-pox, and they should continue coming out till the cow-pox has completed its progress, its vesicle, like any other inflamed part, will become the seat of a small-pox pustule, or the whole vesicle will become purulent, contrary to its legitimate character. In the first case, you may take small-pox matter from the pustule, which, by the adhesive inflammation, will remain distinct from, though seated in part of the vaccine vesicle; and from the other parts of the vesicle you may take vaccine matter, and each will perpetuate its respective morbid poison. If the whole vesicle becomes purulent, it is a variolous pustule, and will inoculate small-pox.

“It was remarked by Dr. Woodville, that if a person is inoculated with small-pox to day, and three or four days after is re-inoculated with the same morbid poison, though the last insertion may remain a smaller pustule than the first, yet both inoculations will arrive at their height at the same time. The same takes place in cow-pox; and also, if a person is inoculated to-day with cow-pox, and three or four days after with small-pox; or to-day with small-pox; and three or four days after with cow-pox; the two insertions, though the last may remain smaller than the first, will mature and scab at the same time.



“ By these facts it appears, first, that a marked kind of small-pox may be perpetuated. If, therefore, the cow-pox is a marked kind of small-pox, there can be no reason why it should not have been perpetuated with its true character; and that the cow-pox is such, appears, secondly, by its not being interrupted by, and not interrupting the progress of small-pox, and by both retaining their respective laws and characters at the same time, whether inoculated separately in different subjects, or in the same: or if each has been inoculated in the same subject at different times, the consequence is similar to the inoculation of either one, at different times.

“ These experiments have been repeated so often, as to leave no question concerning the law. The same experiments have been repeatedly tried with small-pox and varicella, with small-pox and measles, and also with cow-pox and each of the others; yet those interruptions have always followed which have been remarked in the early part of the work.

“ As, therefore, a marked variety of small-pox is capable of preserving its distinct character under inoculation, there seems no reason why the cow-pox should not be among such varieties; and as any of the known varieties will destroy the susceptibility to the disease in all other forms, so there is no reason why cow-pox, if among the varieties, should not do the same; and there is the more reason to expect this, because, contrary to any other morbid poisons, the action of small-pox and cow-pox is maintained at the same time in different parts of the same constitution, subject respectively to similar laws, whether only one or both of them are applied in any variety of forms.

“ It may be said that small-pox is an eruptive disease, whilst cow-pox, though affecting the constitution, is only confined in its local action to a single part. But small-pox is sometimes,

we have seen, equally confined in its local action, and principally in those cases in which its appearance most resembles cow-pox. It is not less certain that cow-pox, on some occasions, produces secondary eruptions. Besides the cases I have seen myself, the Rev. Mr. Holt\* gives an account of a full eruption of vesicles, which had the same properties of contagion as the inoculated part. The Rev. Mr. Fermor† saw a few scattered in different parts. Dr. Woodville first remarked that they appear at the time the inoculated part has formed a scab, which was not the case with those which, in his earlier experiments, proved to be variolous. This appearance after the disease is considered as complete, as well as the few instances in which they occur may be the reason why they are so rarely seen. Dr. Jenner describes them as occurring sometimes in persons vaccinated whilst under herpes, in which case they occupy the places before covered with herpetic blotches: he informs me likewise, that he has inoculated from a secondary pustule on the knee. In the small-pox hospital we have had several such opportunities; but they are certainly rare, compared with the numbers vaccinated. However, the last week has furnished us with a secondary pustule in the neck, the fluid from which is now producing the legitimate character in the arms of two different subjects.

“The laws above mentioned might be unsatisfactory to prove the security of cow-pox, if they were not confirmed by irresistible facts; but the two together place the result with me beyond a doubt. That small-pox has occurred after cow-pox cannot be questioned, nor that it has occurred after small-pox. That fatal cases may have occurred, and others in which herpetic diseases may have followed vaccination, is what I am not disposed to doubt. But it is strange that the strong recommendations of some of its votaries, who have more zeal than wisdom, should have made the sober part of

\* Med. and Phys. Journal.

† Ibid.

us forget, that both inoculation and vaccination are only submitted to in order to avoid greater evils. We know that men have been poisoned by being sucked by leeches; are we, on that account, to refuse their use? We know that suppurations in the joints have followed inoculation for small-pox: but how much more frequent has it occurred under the casual disease? Cow-pox has never been accused of more than a few accidental herpetic eruptions, which very rarely occur, and which, when left to themselves, have for the most part subsided spontaneously.

“ Respecting the laws of the two, they are certainly more permanently different, than between any other varieties of small-pox. But this is all we can urge. The cow-pox has never proved contagious by effluvia. The cow-pox is unattended with a slough, which never fails to attend every variolous insertion. But though the cow-pox does not induce slough, the action excited by it is not merely the elevation of the cuticle into a bladder: it extends to the cellular substance, without destroying its texture. Though the fat seems absorbed, yet the cells remain entire with their former communication, till the lymph reaches a certain defined disk; after this, if the fluid increases, it stretches the cuticle, so that the base is the narrowest part of the whole. In this state, if punctured, the exit of the lymph is very slow, on account of the various little cells communicating with each other in which it is contained. If suffered to remain long unpunctured, the cells will sometimes break, or be absorbed, and the cavity will be less interrupted. Secondary vesicles are entirely under the cuticle, and cannot be distinguished by the eye from the crystallines of small-pox in their early stage: this, and the time of their appearance, prove to a certainty that they cannot be the effect of re-inoculation by the fingers of the patients. The insertions from cow-pox, though subject to some varieties, are more regular in their character than any morbid

poison with which we are acquainted whose progress is so rapid. The insertions from small-pox, though all attended with a slough, are extremely various in their appearance, and sometimes for the first eight days, or longer, cannot easily be distinguished by the eye from the cow-pox. But the ingenious discoverer has marked a permanent difference between the two, which has been too much unnoticed, namely, that in cow-pox the contents of the elevation are always limpid till the scab is formed; the small-pox, on the contrary, always becomes purulent before it dries. This difference arises from the slough in the latter, which renders suppuration necessary for dislodging the dead part. The cicatrix is a general but not a certain criterion. In the small-pox it is either smooth or corrugated, depending on the manner in which the slough has cast off: in the cow-pox it is either beset with smooth indentations, or consists of one or two smooth depressions, depending on the progress of the vesicle. If the cellular substance remains unbroken, the surface will remain marked, as the fat has been absorbed to admit the fluid into the cells. If the inflammation has induced more violence, so that the cells themselves are broken or absorbed, the surface will be more even.

“ As to every other mark of perfect or imperfect vaccination, nothing has been added since the discoverer published his ‘Enquiry.’ There is no mode of judging of vaccination but by the progress of the vesicle, by its contents, its scab, and cicatrix: and though the last may prove a confirmation when regular, it is not to be considered, when irregular,\* as a sufficient proof of want of security. The vast numbers we have tested in every possible way at the hospital, prove to a conviction, that whatever objection may be made by the enemies of vaccination, or whatever doubts and anxieties may be nursed by its over-zealous friends, the practice must ultimately bear down all opposition; and the arguments against it will

\* Nor when wanting. E.

be forgotten, or only recollected, like the early pamphlets against variolous inoculation."

*Med. and Chir. Rev.*



*Remedy in Chronic Ophthalmia.\**

An excellent application here recommended in chronic ophthalmia is the following:—two drachms of the *spiritus volatilis aromaticus*, and two ounces of boiling water, put into a vessel capable of holding three ounces; then wrapping the vessel in a hot cloth, and conducting the vapour to the eye by means of a small funnel, or by merely bringing the vessel close to the eye. This should be repeated three or four times a day, for at least half an hour, and the eyelids and eyebrows gently rubbed with the same spirit.

*Med. and Chir. Rev.*



*Artificial Pupil.†*

In cases of the closure of the pupil, an effect which often follows the operation for the cataract, the author operates in a manner different from that recommended by other writers. Finding that the artificial pupil made in the ordinary way often closed again after a time; and observing on different occasions of wounds and operations, that the margin of the iris was readily detached from the ciliary ligament, M. Scarpa determined to make an artificial pupil in this way without previ-

\* Scarpa on the principal diseases of the eyes.

† From the same.

ously dividing the cornea, as is done in the common mode of operating. This he effects by perforating, with a fine, straight couching needle, the sclerotic coat at the external angle of the eye, about two lines from the union of the sclerotica with the cornea, carrying the point of the needle as far as the upper and internal part of the margin of the iris, that is, on the side next the nose. The instrument is then made to pierce the upper part of the internal margin of the iris, close to the ciliary ligament, until its point is just perceptible in the anterior chamber of the aqueous humour. It should then be pressed upon the iris from above downward, and towards the external angle, so as to tear down the margin of the iris from its attachment above, which may be done to any extent that is desired.

The subject of cataract is treated of at considerable length. A parallel is drawn between the two methods of operating, by *extraction* and by *depression*, and the preference decidedly given to the latter, the advantages peculiar to which are pointed out. This subject has been lately so ably discussed by our ingenious countryman, Mr. Hey of Leeds, that we shall beg leave to refer our readers to his treatise, a pretty full account of which will be found in our Review.\*

*Med. and Chir. Rev.*



*Dubois's Case of a Calculus in the Bladder.*

A case was lately communicated to the society of medicine at Paris, by professor *Dubois*, of a child three years old, that had suffered for many months under pain and difficulty of making water, which led to the supposition of the existence

\* See *Med. and Chir. Rev.* vol. x. p. 66.

of a calculus in the bladder. Upon sounding him, a stone was actually felt, but it was judged by M. *Dubois* to be of so small a size as not to require the operation of lithotomy. On examination some weeks after, the stone was found to have entered the urethra, and was wedged in so firmly, as not to be capable of being thrust back by any instrument. The operation was still delayed; when, one night, the patient complaining of excruciating pain, and the father of the child judging the pain to arise from the urine being suppressed by the calculus, it occurred to him to try the effect of suction. He therefore applied his mouth to the penis of the boy, and sucked strongly. This, at first, seemed to aggravate the pain; but in a little while the stone passed suddenly forward into the mouth, and was immediately followed by a gush of urine.

*Med. and Chir. Review.*



## REVIEW.

*An essay on the truth of Physiognomy, and its application to Medicine.* By RICHARD BROWN, A. M. of Alexandria, Honorary Member of the Philadelphia Medical Society, and Member of the Lyceum.

This essay constitutes one of the few inaugural dissertations, in the university of Pennsylvania, for the year 1807, of the merits of which the public have been enabled to judge. Owing to a regulation recently entered into, relative to the mode of conferring degrees in medicine, most of the other dissertations which should have appeared at the same time with the present one, have been withheld from the press. The professors alone have been favoured with a perusal of them, while all other persons are left to mere conjecture with regard to their worth:



Respecting the wisdom of this measure it does not belong to us positively to decide. Time and experience, the only unerring tests of all rules, and all establishments, must be also the ultimate arbiters of this. We confess that our fears predominate over our hopes respecting their decision in the present instance. We deem it, however, a subject of congratulation to the friends of literature and science, as well as to Dr. Brown himself, that the regulation here referred to has had no effect either in *relaxing the ardour of his spirit of inquiry, in preparing his theses, or in excluding the fruits of this inquiry from the public eye.* For we cannot conceal our apprehensions, that should this regulation continue in force, *such an effect* will be inevitably produced on too many of our future candidates for medical honours.

General physiognomy Dr. Brown defines to be “ a knowledge of the connection that exists between the external or visible appearances, and the internal or invisible qualities of the works of nature.” By human physiognomy, he means, “ a knowledge of the physical, moral and intellectual qualities and endowments of man, derived from an observance of his countenance, person and deportment; a knowledge of what passes within him, founded on an inspection of what appears without.”

The truth of physiognomy is not only denied by many, but all pretensions to it, as an object of pursuit, treated, if not as the dreams of a distempered brain, at least as the wanderings of an eccentric imagination. A knowledge of this induced Dr. Brown to enter on a full vindication of this branch of science from such unfounded and illiberal charges. In this part of his essay, he has displayed great reading and observation, connected with a strength of judgment, and powers of reasoning, that would do honour to a man of much riper years.



It is under this head in particular, that our youthful author has taken an uncommonly wide and variegated range. For, though, as he justly observes, "physiognomy is generally treated of only in relation to beings possessed of life; yet it is an error to restrict it within so narrow a compass. It greatly transcends the bounds of the animal and vegetable kingdoms, and may be said, without an hyperbole, to be commensurate with those of nature herself." Conformably to this opinion, his first attempt is to establish the truth of what he calls "the physiognomy of the heavens." From this daring height he suddenly descends, though not with Icarian fate, to the physiognomy of the earth, where equal success awaits his labours. He then passes on to the physiognomy of vegetables, from whence he again hastens to a still richer and more favourite field, "the physiognomy of animated nature." Under this head we need offer no apology to the reader, for laying before him the two following paragraphs, in our author's own words.

"When we hastily glance our eyes over the fields, forests, marshes, and mountains, we behold them peopled with animals, differing from each other in appearance and in character. Some of these animals subsist on the flesh of their fellow-creatures, others on vegetables; some of them procure their food by stratagem, and others by strength. They are all perfectly calculated by their forms, structures and dispositions for the particular modes of life they are severally destined to pursue. But this is not all. The general aspects of many of them convey to the minds even of common observers, a correct knowledge of the propensities, qualities, and dispositions, which they possess.

"Who under the commanding figure and aspect of the eagle, would expect to find the gentleness and timidity of the dove? or the bold and generous daring of the lion under the shrinking appearance of the hind? Is not savage fierceness

strongly pourtrayed in the countenance of the tiger, treachery and voracious cruelty in the appearance of the wolf, honesty, patience and moderation in that of the cow, insidious cunning in the fox, magnanimous courage in the horse, and the most inoffensive innocence in the aspect of the lamb? Is not the appearance of the hog expressive of sloth and stupidity, that of the baboon or the monkey of acuteness and drollery, and that of the stag of great timidity united to an equal degree of swiftness and strength? We may observe in general that the aspect of carnivorous animals, whether birds or beasts, bespeaks a ferocity and an unrelentingness of disposition, while that of the herbivorous or graminivorous is characteristic of more mildness and docility of temper. And such in reality is the difference that exists between the characters and dispositions of these two tribes of animals."

Our author next proceeds to consider the physiognomy of man. In his attempt to prove the truth of this branch of his subject, he has exhibited not only an able but a very elegant specimen of learning and intellect. The writers whom he quotes are of the first rank in literature both ancient and modern, and he has shewn himself to possess a correct, familiar and commanding knowledge of them. His acquaintance with the Greek mythology in particular, as well as the most distinguished of the Greek, Roman and Italian poets, appears to singular advantage. But, for the support of his doctrine, he does not depend exclusively on the authority of others. Perhaps his weightiest evidence is derived from the resources of his own observation. Let us listen to some of his remarks on this head.

"I assert it as a fact, and challenge the disbelievers in physiognomy to prove to the contrary, that the world has never yet produced a truly great man, clothed in an aspect of insignificance. When preeminent and striking qualities, whe-

ther good or bad, predominate in the mind, they are uniformly accompanied by some corresponding marks in the countenance. Was there ever an Alexander or a Cæsar with a timid look? a Cicero or a Newton with the vacant face of an idiot? a Nero or a Domitian with the calmness and serenity of virtue or benevolence on their brows? or a Marcus Aurelius, or a Washington with a countenance, marked by weakness or clouded with guilt? It is neither rash nor presumptuous to pronounce, with confidence, that there never was. A Centaur itself, composed of the head and body of a man, and the extremities of a horse, would not be a more perfect monster in nature, than a prince, possessing the mental energies of a Buonaparte, with the weak and vapid countenance of a Henry VI. These are truths which reason herself inculcates on us through the purest of channels, observation and experience."

Again: "A tacit confidence in the truth of physiognomy, constitutes the basis of all sudden attachments between the sexes, as well as of many friendships between individuals of the same sex. On these occasions, it is not merely beauty of form, complexion, or features; it is the soul—illumined female face, the countenance of soft sensibility, of inexpressible sweetness, or of brilliant vivacity, that first lights up, as by the breath of magic, the tender flame in the bosom of the lover. In many instances also, sudden and lasting friendships between men are founded, at first, on nothing more than an intuitive perception in the parties, of a mutual correspondence or affinity of mind, founded entirely on their respective appearances. I presume that very few have arrived at the age of maturity, without experiencing in their own persons, something of the truth of this remark. For it appears almost impossible for any individual to mingle in society, without discovering some other individual, with whom at first sight, he does not feel a secret desire to cultivate an acquaintance."

Dr. Brown now passes on to treat of *medical physiognomy*, which he defines "a knowledge or discovery of the internal diseased affections of the system, derived from an examination of the external appearances exhibited by the sick." For the truth of this branch of knowledge, he appeals to books as well as to observation, and points out in a very clear and satisfactory manner, the advantages which a physician must derive from an acquaintance with it.

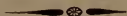
He goes on to treat of the temperaments of man. These he considers as natural or constitutional predispositions to certain forms or states of disease, in preference to others. He thinks there are but three temperaments distinctly marked by external appearances, namely, the *sanguineous*, the *bilious*, and the *lymphatic*. He enumerates, with correctness and perspicuity, the visible marks of complexion, countenance, and person, considered as characteristic of these several temperaments, and mentions the diseases to which individuals possessing them are more peculiarly liable. What is called the *melancholic* he does not regard as an original temperament at all, but as an actual state of disease connected with the bilious temperament. This diseased state he supposes to consist in visceral obstructions.

Our author concludes his interesting dissertation by enumerating various physiognomical signs that occur in diseases, annexing to each sign its natural expression or indication, relative to the death or recovery of the sick. He acknowledges that, under this head of his subject, he derives his principal information from Hippocrates, whose order and method, in his prognostics, he accurately pursues. The particular sources from which these physiognomical signs are drawn, are the *countenance*, the *tongue*, the *teeth*, the *respiration*, the *decubitus*, and the *extremities* of the sick.

But numerous as the excellencies and beauties of this dissertation are, it cannot be said to be altogether free from faults. Dr. Brown seems to alledge, that the Cretins of the Alps, and the Albinos, are of the same tribe or description of people. This we believe to be a mistake, though it, no doubt, arose more from inadvertence than from a want of correct knowledge on the subject. We, notwithstanding, concur in opinion with the Doctor on the principal point he wishes to establish, namely, that both Cretins and Albinos possess, in general, the lymphatic temperament in a very high degree.

Another objection we would urge is, that the dissertation is too concise under the immediate head of *medical physiognomy*. This, though, perhaps, not in all respects the most interesting, is certainly the principal and most useful division of the whole subject. It is a matter of regret, therefore, that our learned and ingenious author has not dwelt on it more at large, and treated it more in detail. We hope, however, that he will make amends for this in future, by continuing to pursue a subject, to which he is so well calculated to do ample justice.

On the whole, we cannot forbear speaking of Dr. Brown as a young author of very superior promise. Should he persevere in the cultivation of those talents for research and fine writing, with which nature has so liberally endowed him, he cannot fail to do honour to himself, his profession, and his country. His dissertation "on the truth of physiognomy, and its application to medicine," is most cordially recommended as a delightful repast to the lovers of science and polite literature.



## CORRESPONDENCE.

SIR,

One of your correspondents, acknowledging his fondness of prying into the *arcana naturæ*, wishes the solution of a question that I have been often asked; why the *montes pectoris* have lately acquired such a prominency?

This, our enquirer seems to consider as a phenomenon unnoticed in his youthful days, throwing, at the same time, some hints upon several causes to which it might be ascribed. As the beauties of nature have always commanded my attention, I beg, *sans aller trop loin*, to participate with him my observations.

Like the heavenly bodies, those globes have their apogee and perigee, eclipse and apparition. Happy are we to live under the present phasis; may it last as long as the sun continues to shine upon our planet. I have, some years ago, predicted with as much certainty, as I might have announced an eclipse of the moon, their appearance; the verging to natural good taste being a symptom that cannot escape an observing eye; for I must tell, by the bye, that they were in existence then, as well as at this present time, but inhumanly suffering under the oppression of an erroneous, I may say, a deleterious prejudice.

The loose, elegant and natural dress, but not "the loose manners of the age," has given to nature the opportunity of re-conquering her rights overpowered by the despotism of error. The most enchanting being was for many years past so much fettered and bundled up, that if any of our ancestors were coming now to visit us, they would undoubtedly think, that a new creation has taken place on the face of the earth. I am almost ashamed to recollect, that the American fair sex, who has a claim to, and indubitably deserves *la pomme d'or*,

was so much disguised, by the ungraceful dress of former times, that foreigners remarked, that our women had nothing prominent but the shoulders. But we are now under great obligation to them, for having restored every thing to its right place.

Love tales and novels were read by our young girls many years ago ; they can produce no other effect now than they did then ; but if I could think that they might have the power of improving, *ou le moule, ou l'ouvrage de la nature*, I would immediately give them the first place in our *materia medica*.

Supposing for a moment that the reading of lascivious books increases some secretions, admitting likewise all the power of sympathy, and granting every conclusion of ingenious young men upon the arcana of generation ; *experience*, which I beg leave to introduce as deserving an ample share of credit, teaches us, that *les femmes qui sont les plus lascives, sont celles qui ont le moins de sein*.

DELTA.

J. R. COXE, M. D.



Dr. Woodhouse informed the editor, that he has received a number of specimens of molybdena from Chester county, Pennsylvania. It is found mixed with iron and copper pyrites, and is deposited on quartz. Its specific gravity, at 62° of Fahrenheit's thermometer is 4,648.

The same gentleman has obtained honey of an exquisite taste, which deposits crystals of brown sugar, by digesting alcohol, on the ripe fruit of the *Diospyros Virginiana*, American prune, date plum or persimmon tree, and evaporating the solution. This tree is a native of all the southern states. The juice of the unripe fruit, possesses more astringency, and contains a greater quantity of the tanning principle, than any vegetable in the world.



The Editor has received through the attention of Dr. John C. Warren, recording secretary of the Massachusetts medical society, the second number, (part first) of a work published by the Society, entitled "Medical papers, communicated to the Massachusetts Medical Society," &c.

Not having seen the first number, which probably contains the plan intended to be pursued in the prosecution of the work; the Editor can only congratulate his brethren, upon the energy which appears to pervade the medical profession throughout America. The contemplation of the benefits likely to be derived, from a publication conducted under the sanction of so respectable an institution, cannot but prove highly satisfactory, and we sincerely trust that nothing may obstruct its continuance.

The present number consists of the following very interesting communications, viz.

1. Medical discourse on several narcotic plants, by Dr. Fisher.
2. Case of ruptured uterus, by Dr. Prescott.
3. Of dislocation and fracture, by Dr. Hazaltine.
4. Of preternatural retention of urine, by Dr. Thatcher.
5. History of a wound in the femoral artery, by Dr. Warren, jun.
6. Some observations on worms infesting the human body, by Dr. Fisher.

The Names of the Officers of the Society, for 1806, are prefixed, viz.

John Warren, M. D. *President*; Joshua Fisher, M. D. *Vice-President*.

*Censors.* Lemuel Hayward, A. M. Thomas Welsh, A. M. Aaron Dexter, M. D. Josiah Bartlett, M. D. William Spooner, M. D. James Jackson, M. B. *Treasurer*. Thomas Welsh, A. M. *Corresp. Secretary*, John C. Warren, M. D. *Rec. Secretary*. John Fleet, M. D. *Librarian*.

*Censors of the District Society of Worcester.*

Oliver Fisk, John Green, John Frink, Jonathan Osgood, Thomas Babbit.

*Counsellors.*

*Suffolk.*—Isaac Rand, Lemuel Hayward, John Jeffries, Thomas Kast, John Warren, Thomas Welsh, Aaron Dexter, William Eustis, William Spooner, John Fleet, Isaac Rand, jun. Thomas Dunforth, James Jackson, John C. Warren, John C. Howard.

*Essex.*—Edward A. Holyoke, Micajah Sawyer, Joshua Fisher, Thomas Kittredge, Benjamin L. Oliver, John D. Treadwell.

*Middlesex.*—Josiah Bartlett, John Brooks, Isaac Hurd, Oliver Prescott, William Gammege.

*Worcester.*—Oliver Fisk, Israel Atherton, Jonathan Osgood.

*Hampshire.*—Ebenezer Hunt, Henry Wells, Chauncey Brewer.

*Berkshire.*—Erastas Surgeant, Timothy Childs.

*Norfolk.*—Cotton Tufts, Amos Holbrook, John Bartlett.

*Plymouth.*—James Thatcher, Gad Hitchcock.

*Bristol.*—William Baylies.

*Barnstable and Nantucket.*—Samuel Savage.

*Maine.*—Daniel Cony, Nathaniel Coffin, A. R. Mitchell, Shirley Erving, Samuel Adams.

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THOMAS DOBSON will speedily publish an abridgment of Baudelocque's Midwifery, by Dr. Dewees of this city.

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Erratum.—page xc line 23. *Acarus Firo*—read *Acarus Siro*.



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# MEDICAL AND PHILOSOPHICAL REGISTER.

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## FOREIGN AND DOMESTIC.

*Report of the Royal College of Physicians of London, on Vaccination. With an Appendix, containing the Opinions of the Royal College of Physicians of Edinburgh and Dublin; and of the Royal Colleges of Surgeons of London, of Dublin, and of Edinburgh.*

THE Royal College of Physicians of London, having received his Majesty's commands, in compliance with an address from the House of Commons, "to inquire into the state of Vaccine inoculation in the United Kingdom, to report their opinion and observations upon that practice, upon the evidence which has been adduced in its support, and upon the causes which have hitherto retarded its general adoption;" have applied themselves diligently to the business referred to them.

Deeply impressed with the importance of an inquiry which equally involves the lives of individuals and the public prosperity, they have made every exertion to investigate the subject fully and impartially. In aid of the knowledge and experience of the members of their own body, they have applied separately to each of the Licentiates of the college; they have corresponded with the Colleges of Physicians of Dublin and Edinburgh; with the Colleges of Surgeons of London, Edinburgh, and Dublin; they have called upon the societies established for vaccination, for an account of their practice, to what extent

it has been carried on, and what has been the result of their experience ; and they have, by public notice invited individuals to contribute whatever information, they had severally collected. They have in consequence been furnished with a mass of evidence communicated with the greatest readiness and candour, which enables them to speak with confidence upon all the principal points referred to them.

I. During eight years which have elapsed since Dr Jenner made his discovery public, the progress of vaccination has been rapid, not only in all parts of the United Kingdom, but in every quarter of the civilized world. In the British islands some hundred thousands have been vaccinated ; in our possessions in the East Indies upwards of 800,000, and among the nations of Europe the practice has become general. Professional men have submitted it to the fairest trials, and the public have, for the most part, received it without prejudice. A few indeed have stood forth the adversaries of vaccination, on the same grounds as their predecessors who opposed the inoculation for the small-pox, falsely led by hypothetical reasoning in the investigation of a subject which must be supported, or rejected, upon facts and observation only. With these few exceptions, the testimony in favour of vaccination has been most strong and satisfactory, and the practice of it, though it has received a check in some quarters, appears still to be upon the increase in most parts of the United Kingdom.

II. The College of Physicians, in giving their observations and opinions on the practice of vaccination, think it right to premise, that they advance nothing but what is supported by the multiplied and unequivocal evidence which has been brought before them, and they have not considered any facts as proved, but what have been stated from actual observation.

Vaccination appears to be in general perfectly safe; the instances to the contrary being extremely rare. The disease excited by it is slight, and seldom prevents those under it from following their ordinary occupations. It has been communicated with safety to pregnant women, to children during dentition, and in their earliest infancy; in all which respects it possesses material advantages over inoculation for the small-pox; which, though productive of a disease generally mild, yet sometimes occasions alarming symptoms, and is in a few cases fatal.

The security derived from vaccination against the small-pox, if not absolutely perfect, is as nearly so as can perhaps be expected from any human discovery; for amongst several hundred thousand cases, with the results of which the College have been made acquainted, the number of alleged failures have been surprisingly small, so much so, as to form certainly no reasonable objection to the general adoption of vaccination; for it appears that there is not nearly so many failures in a given number of vaccinated persons, as there are deaths in an equal number of persons inoculated for the small-pox. Nothing can more clearly demonstrate the superiority of vaccination over the inoculation of the small-pox, than this consideration; and it is a most important fact, which has been confirmed in the course of this inquiry, that in almost every case, where the small pox has succeeded vaccination, whether by inoculation or by casual infection, the disease has varied much from its ordinary course; it has neither been the same in violence, nor in the duration of its symptoms, but has, with very few exceptions, been remarkably mild, as if the small-pox had been deprived, by the previous vaccine disease, of all its usual malignity.

The testimonies before the College of Physicians are very decided in declaring, that vaccination does less mischief to the

constitution, and less frequently gives rise to other diseases, than the small-pox, either natural or inoculated.

The College feel themselves called upon to state this strongly, because it has been objected to vaccination, that it produces new, unheard-of, and monstrous diseases. Of such assertions no proofs have been produced, and, after diligent inquiry, the College believe them to have been either the inventions of designing, or the mistakes of ignorant men. In these respects then, in its mildness, its safety, and its consequences, the individual may look for the peculiar advantages of vaccination. The benefits which flow from it to society are infinitely more considerable; it spreads no infection, and can be communicated only by inoculation. It is from a consideration of the pernicious effects of the small-pox, that the real value of vaccination is to be estimated. The natural small-pox has been supposed to destroy a sixth part of all whom it attacks; and that even by inoculation, where that has been general in parishes and towns, about one in 300 has usually died. It is not sufficiently known, or not adverted to, that nearly one-tenth, some years more than one-tenth of the whole mortality in London, is occasioned by the small-pox; and however beneficial the inoculation of the small-pox may have been to individuals, it appears to have kept up a constant source of contagion, which has been the means of increasing the number of deaths by what is called the natural disease. It cannot be doubted that this mischief has been extended by the inconsiderate manner in which great numbers of persons, even since the introduction of vaccination, are still every year inoculated with the small-pox, and afterwards required to attend two or three times a week at the places of inoculation, through every stage of their illness.

From this, then, the public are to expect the great and uncontroverted superiority of vaccination, that it communicates

no casual infection, and, while it is a protection to the individual, it is not prejudicial to the public.

III. The College of Physicians, in reporting their observations and opinions on the evidence adduced in support of vaccination, feel themselves authorised to state that a body of evidence so large, so temperate, and so consistent, was perhaps never before collected upon any medical question. A discovery so novel, and to which there was nothing analogous known in nature, though resting on the experimental observations of the inventor, was at first received with diffidence: it was not, however, difficult for others to repeat his experiments, by which the truth of his observations was confirmed; and the doubts of the cautious were gradually dispelled by extensive experience. At the commencement of the practice, almost all that were vaccinated were afterwards submitted to the inoculation of the small-pox; many underwent this operation a second, and even a third time, and the uniform success of these trials quickly bred confidence in the new discovery. But the evidence of the security derived from vaccination against the small-pox does not rest alone upon those who afterwards underwent variolous inoculation, although amounting to many thousands; for it appears, from numerous observations communicated to the College, that those who have been vaccinated are equally secure against the contagion of epidemic small-pox. Towns indeed, and districts of the country, in which vaccination had been general, have afterwards had the small-pox prevalent on all sides of them without suffering from the contagion. There are also in the evidence a few examples of epidemic small-pox having been subdued by a general vaccination. It will not, therefore, appear extraordinary, that many who have communicated their observations should state, that though at first they thought unfavourably of the practice, experience had now removed all their doubts.

It has been already mentioned, that the evidence is not universally favourable, although it is in truth nearly so, for there are a few who entertain sentiments differing widely from those of the great majority of their brethren. The College, therefore, deemed it their duty, in a particular manner, to inquire upon what grounds and evidence the opposers of vaccination rested their opinions. From personal examination, as well as from their writings, they endeavoured to learn the full extent and weight of their objections. They found them without experience in vaccination, supporting their opinions by hearsay information, and hypothetical reasoning, and, upon investigating the facts which they advanced, they found them to be either misapprehended or misrepresented, or that they fell under the description of cases of imperfect small-pox, before noticed, and which the College have endeavoured fairly to appreciate.

The practice of vaccination is but of eight years standing, and its promoters, as well as opponents, must keep in mind, that a period so short is too limited to ascertain every point, or to bring the art to that perfection of which it may be capable. The truth of this will readily be admitted by those acquainted with the history of inoculation for the small-pox. Vaccination is now, however, well understood, and its character accurately described. Some deviations from the usual course have occasionally occurred, which the author of the practice has called spurious cow-pox, by which the public have been misled, as if there were a true and false cow-pox; but it appears, that nothing more was meant, than to express irregularity or difference from that common form and progress of the vaccine pustule from which its efficacy is inferred. Those who perform vaccination ought therefore to be well instructed, and should have watched with the greatest care the regular progress of the pustule, and learnt the most proper time for taking the matter. There is little doubt that some of the

failures are to be imputed to the inexperience of the early vaccinators, and it is not unreasonable to expect that farther observations will yet suggest many improvements that will reduce the number of anomalous cases, and furnish the means of determining, with greater precision, when the vaccine disease has been effectually received.

Though the College of Physicians have confined themselves in estimating the evidence to such facts as have occurred in their own country, because the accuracy of them could best be ascertained, they cannot be insensible to the confirmation these receive from the reports of the successful introduction of vaccination, not only into every part of Europe, but throughout the vast continents of Asia and America.

IV. Several causes have had a partial operation in retarding the general adoption of vaccination; some writers have greatly undervalued the security it affords, while others have considered it to be of a temporary nature only; but if any reliance is to be placed on the statements which have been laid before the College, its power of protecting the human body from the small-pox, though not perfect indeed, is abundantly sufficient to recommend it to the prudent and dispassionate, especially as the small-pox, in the few instances where it has subsequently occurred, has been generally mild and transient. The opinion that vaccination affords but a temporary security is supported by no analogy in nature, nor by the facts which have hitherto occurred. Although the experience of vaccine inoculation be only of a few years, yet the same disease, contracted by the milkers of cows, in some districts has been long enough known to ascertain that in them, at least the unsusceptibility of the small-pox contagion does not wear out by time. Another cause, is the charge against vaccination of producing various new diseases of frightful and monstrous appearance.



Representations of some of these have been exhibited in prints in a way to alarm the feelings of parents, and to infuse dread and apprehension into the minds of the uninformed. Publications with such representations have been widely circulated, and though they originate either in gross ignorance, or wilful misrepresentation, yet have they lessened the confidence of many, particularly of the lower classes, in vaccination; no permanent effects, however, in retarding the progress of vaccination, need be apprehended from such causes, for, as soon as the public shall view them coolly and without surprise, they will excite contempt, and not fear.

Though the College of Physicians are of opinion that the progress of vaccination has been retarded in a few places by the above causes, yet they conceive that its general adoption has been prevented by causes far more powerful, and of a nature wholly different. The lower orders of society can hardly be induced to adopt precautions against evils which may be at a distance; nor can it be expected from them, if these precautions are attended with expense. Unless therefore, from the immediate dread of epidemic small-pox, neither vaccination nor inoculation appear at any time to have been general, and when the cause of terror has passed by, the public have relapsed again into a state of indifference and apathy, and the salutary practice has come to a stand. It is not easy to suggest a remedy for an evil so deeply imprinted in human nature. To inform and instruct the public mind may do much, and it will probably be found that the progress of vaccination in different parts of the United Kingdom will be in proportion to that instruction. Were encouragement given to vaccination, by offering it to the poorer classes without expense, there is little doubt but it would in time supersede the inoculation for the small-pox, and thereby various sources of variolous infection would be cut off; but till vaccination becomes general, it will be impossible to prevent the constant recurrence of the natural



small-pox by the means of those who are inoculated, except it should appear proper to the legislature to adopt, in its wisdom, some measure by which those who still, from terror or prejudice, prefer the small-pox to the vaccine disease, may, in thus consulting the gratification of their own feelings, be prevented from doing mischief to their neighbours.

From the whole of the above considerations, the College of Physicians feel it their duty strongly to recommend the practice of vaccination. They have been led to this conclusion by no pre-conceived opinion, but by the most unbiassed judgment, formed from an irresistible weight of evidence which has been laid before them. For when the number, the respectability, the disinterestedness, and the extensive experience of its advocates, is compared with the feeble and imperfect testimonies of its few opposers; and when it is considered that many, who were once adverse to vaccination, have been convinced by further trials, and are now to be ranked among its warmest supporters, the truth seems to be established as firmly as the nature of such a question admits; so that the College of Physicians conceive that the public may reasonably look forward with some degree of hope to the time when all opposition shall cease, and the general concurrence of mankind shall at length be able to put an end to the ravages at least, if not to the existence, of the small-pox.

LUCAS PEPYS, *President.*

Royal College of Physicians  
10th April 1807.

J<sup>A</sup>. HERVEY, *Register.*

VOL. IV.

## APPENDIX.

## No. I.

*To the Royal College of Physicians of London.*

GENTLEMEN,

I AM ordered by the King and Queen's College of Physicians, in Ireland, to thank the Royal College of Physicians, of London, for the communication they have had the honour to receive from them, of certain propositions relative to vaccination, whereon His Majesty has been pleased to direct an inquiry to be instituted, and in the prosecution of which, the co-operation of the College in Ireland is requested.

And I am directed to acquaint you, that the said College having referred the investigation of these propositions to a committee, have received from them a report, of which the enclosed is a copy; and that they desire the same may be considered as containing their opinion upon the subject.

I have the honour to be, Gentlemen,

Your most obedient humble Servant,

HUGH FERGUSON, *Register.*

By order of the King and Queen's College of  
Physicians in Ireland.

*Dublin, Nov. 11th, 1806.*

“The practice of Vaccination was introduced into this city about the beginning of the year 1801, and appears to have made inconsiderable progress at first. A variety of causes ope-

rated to retard its general adoption, amongst which the novelty of the practice, and the extraordinary effects attributed to vaccination, would naturally take the lead.

“Variolous inoculation had been long, almost exclusively, in the hands of a particular branch of the profession, whose prejudices and interests were strongly opposed to the new practice ; and by their being the usual medical attendants in families, and especially employed in the diseases of children, their opinions had greater effect upon the minds of parents. The small-pox is rendered a much less formidable disease in this country by the frequency of inoculation for it, than it is in other parts of His Majesty’s dominions, where prejudices against inoculation have prevailed ; hence parents, not unnaturally, objected to the introduction of a new disease, rather than not recur to that, with the mildness and safety of which they were well acquainted.

“In the beginning of the year 1804, the Cow-pox Institution was established, under the patronage of the earl of Hardwicke, and it is from this period that we may date the general introduction of vaccination into this city, and throughout all parts of Ireland.

“The success of the institution, in forwarding the new practice, is to be attributed in a great measure to the respectability of the gentlemen who superintend it, and to the diligence, zeal, and attention of Dr. Labatt, their Secretary and Inoculator. In order to shew the progress which has been made in extending vaccination, your committee refer to the reports of the Cow-pox institution for the last two years, and to extracts from their Register for the present year.

Year.	Patients Inoculated.	Packets issued to Practitioners in general.	Packets to Army Surgeons.
1804 -	578	776	236
1805 -	1,032	1,124	178
1806 -	1,356	1,340	220
Total -	2,966	3,240	634

“In the above statement, the numbers are averaged to the end of the present year, on the supposition of patients resorting to the institution as usual. The correspondence of the institution appears to be very general throughout every part of Ireland, and by the accounts received, as well from Medical Practitioners as others, the success of vaccination seems to be uniform and effectual. At the present period, in the opinion of your Committee, there are few individuals in any branch of the profession, who oppose the practice of vaccination in this part of His Majesty’s dominions.

“It is the opinion of your Committee, that the practice of Cow-pox Inoculation is safe, and that it fully answers all the purposes that have been intended by its introduction. At the same time, your Committee is willing to allow that doubtful cases have been reported to them as having occurred, of persons suffering from Small-pox, who had been previously vaccinated. Upon minute investigation, however, it has been found that these supposed instances originated generally in error, misrepresentation, or the difficulty of discriminating between Small-pox and other eruptions, no case having come to the knowledge of your Committee, duly authenticated by respectable and competent judges, of genuine Small-pox succeeding the regular Vaccine disease.

“The practice of Vaccination becomes every day more extended; and, when it is considered that the period at which it

came into general use in Ireland is to be reckoned from so late a date, your Committee is of opinion, that it has made already as rapid a progress as could be expected.

(Signed)

“ JAMES CLEGHORN.

“ DANIEL MILLS.

“ HUGH FERGUSON.”

No. II.

*Physicians Hall, Edinburgh Nov. 26th, 1807.*

GENTLEMEN,

THE Royal College of Physicians of Edinburgh have but little opportunity themselves of making observations on Vaccination, as that practice is entirely conducted by Surgeon Apothecaries, and other Medical Practitioners not of their College, and as the effects produced by it are so inconsiderable and slight, that the aid of a Physician is never required.

The College know that in Edinburgh it is universally approved of by the profession, and by the higher and middle ranks of the community, and that it has been much more generally adopted by the lower orders of the people than ever the Inoculation for Small-pox was, and they believe the same to obtain all over Scotland.

With regard to any causes which have hitherto prevented its general adoption, they are acquainted with none, except the negligence or ignorance of parents among the common people, or their mistaken ideas of the impropriety or criminality of being accessory to the production of any disease among their children, or the difficulty or impossibility, in some of our

country districts of procuring Vaccine matter, or a proper person to inoculate.

The evidence in favour of Vaccination appeared to the Royal College of Physicians of Edinburgh so strong and decisive, that in May last, they spontaneously and unanimously elected Dr. Jenner an Honorary Fellow of their College;—a mark of distinction which they very rarely confer, and which they confine almost exclusively to Foreign Physicians of the first eminence.

They did this with a view to publish their opinion with regard to Vaccination, and in testimony of their conviction of the immense benefits which have been, and which will in future be derived to the world, from Inoculation for the Cow-pox, and as a mark of their sense of Dr. Jenner's very great merits and ability in introducing and promoting this invaluable practice.

I have the honour to be, Gentlemen,

Your most obedient humble Servant,

TH. SPENS. C. R. M. ED. PR.

To the Royal College of Physicians of London.

### No. III.

At a Special Court of Assistants of the Royal College of Surgeons, convened by order of the Master, and holden at the College on Tuesday the 17th day of March 1807;

Mr. Governor LUCAS in the Chair:

MR. LONG, as Chairman of the Board of Curators, reported, That the Board are now ready to deliver their report on the subject of vaccination.

It was then moved, seconded, and Resolved, That a Report from the Board of Curators, on the subject of vaccination, which was referred to their consideration by the Court of Assistants, on the 21st day of November last, be now received.

Mr. Long then delivered to Mr. Governor Lucas (presiding in the absence of the Master) a Report from the Board of Curators.

It was then moved, seconded, and Resolved, that the Report delivered by Mr. Long, be now read ; and it was read accordingly, and is as follows :

To the Court of Assistants of the Royal College of Surgeons in London.

THE Report of the Board of Curators, on the subject of Vaccination, referred to them by the Court, on the 21st day of November 1806 ; made to the Court on the 17th of March, 1807.

THE Court of Assistants having received a Letter from the Royal College of Physicians of London, addressed to this College, stating, that His Majesty had been graciously pleased, in compliance with an address from the Honourable House of Commons, to direct his Royal College of Physicians of London to inquire into the state of Vaccination in the United Kingdom, to report their observations and opinion upon that practice, upon the evidence adduced in its support, and upon the causes which have hitherto retarded its general adoption ; that the College were then engaged in the investigation of the several propositions thus referred to them, and requesting this College to co-operate and communicate with them, in order that the report thereupon might be made as complete as possible.

And having, on the 21st day of November last, referred such letter to the consideration of the Board of Curators, with

authority to take such steps respecting the contents thereof as they should judge proper, and report their proceedings thereon, from time to time, to the Court :—The Board proceeded with all possible dispatch to the consideration of the subject.

The Board being of opinion that it would be proper to address circular letters to the Members of this College, with a view of collecting evidence, they submitted to the consideration of the Court, holden on the 15th day of December last, the drafts of such letter as appeared to them best calculated to answer that end; and the same having been approved by the Court, they caused copies thereof to be sent to all the Members of the College in the United Kingdom, whose residence could be ascertained, in the following form: *viz.*

“SIR,

The Royal College of Surgeons being desirous to co-operate with the Royal College of Physicians of London, in obtaining information respecting vaccination, submit to you the following Questions, to which the favour of your Answer is requested.

“By order of the Court of Assistants.

“OKEY BELFOUR, *Secretary.*”

Lincoln's-Inn Fields, Dec. 15th, 1806.

“1st. How many persons have you vaccinated?

“2d. Have any of your patients had the small-pox, after vaccination? In the case of every such occurrence, at what period was the vaccine matter taken from the vesicle? How was it preserved? How long before it was inserted? What was the appearance of the inflammation? And what the interval between vaccination and the variolous eruption?



“ 3d. Have any bad effects occurred in your experience in consequence of vaccination ? And if so, what were they.

“ 4th. Is the practice of vaccination increasing or decreasing in your neighbourhood ; if decreasing, to what cause do you impute it ?”

To such letters the Board have received 426 answers ; and the following are the results of their investigation :

The number of persons, stated in such letters to have been vaccinated, is 164,381.

The number of cases in which small-pox had followed vaccination is 56.

The Board think it proper to remark under this head, that, in the enumeration of cases in which small-pox has succeeded vaccination, they have included none but those in which the subject was vaccinated by the surgeon reporting the facts.

The bad consequences which have arisen from vaccination are, eruptions of the skin in 66 cases, and inflammation of the arm in 24 instances, of which three proved fatal.

Vaccination, in the greater number of counties from which reports have been received, appears to be increasing ; it may be proper however to remark, that, in the Metropolis, it is on the decrease.

The principal reasons assigned for decrease are, Imperfect vaccination ; Instances of small-pox after vaccination ; Supposed bad consequences ; Publications against the practice ; Popular prejudices.

And such Report having been considered, it was moved, seconded, and

Resolved, that the Report now read, be adopted by this Court, as the answer of the Court to the letter of the Royal College of Physicians, of the 23d of October last, on the subject of Vaccination.

Resolved. That a copy of these Minutes and Resolutions, signed by Mr. Governor Lucas (presiding at this Court in the absence of the Master) be transmitted by the Secretary to the Register of the Royal College of Physicians.

(Signed) WM. LUCAS.

No. IV.

*Edinburgh, March 3d, 1807.*

SIR,

I mentioned in my former letter, that I would take the earliest opportunity of laying before the Royal College of Surgeons of Edinburgh, the communication with which the Royal College of Physicians of London had honoured them, on the 23d of October last :

I am now directed by the Royal College to send the following answer on that important subject.

The practice of vaccine inoculation, both in private and at the Vaccine Institution established here in 1801, is increasing so rapidly, that for two or three years past, the small-pox has been reckoned rather a rare occurrence, even amongst the lower order of the inhabitants of this city, unless in some particular

quarters about twelve months ago, and, among the higher ranks of the inhabitants, the disease is unknown.

The Members of the Royal College of Surgeons have much pleasure in reporting, That, as far as their experience goes, they have no doubt of the permanent security against the small-pox which is produced by the constitutional affection of the cow-pox; and that such has hitherto been their success in vaccination, as also to gain for it the confidence of the public, in so much that they have not been required, for some years past, to inoculate any person with small-pox who have not previously undergone the inoculation with the cow-pox.

The Members of the Royal College have met with no occurrence in their practice of cow-pox inoculation which could operate in their minds to its disadvantage, and they beg leave particularly to notice, that they have seen no instance of obstinate eruptions, or of new and dangerous diseases, which they could attribute to the introduction among mankind of this mild preventive of the small pox. The Royal College of Surgeons know of no causes which have hitherto retarded the adoption of vaccine inoculation here; on the contrary, the practice has become general within this city: and from many thousand packets of vaccine matter having been sent by the Members of the Royal College, and the Vaccine Institution here, to all parts of the country, the Royal College have reason to believe that the practice has been as generally adopted throughout this part of the United Kingdom as could have been expected from the distance of some parts of the country from proper medical assistance, and other circumstances of that nature.

I have the honour to be, Sir,

Your most obedient servant,

WM FARQUHARSON.

President of the Royal College and Incorporation  
of Surgeons of Edinburgh.

No. V.

Royal College of Surgeons in Ireland,

*Dublin, Feb. 4th, 1807.*

SIR,

I am directed to transmit to you the enclosed Report of a Committee of the College of Surgeons in Ireland, to whom was referred a letter from the Royal College of Physicians in London, relative to the present state of vaccination in this part of the United Kingdom; and to state, that the College of Surgeons will be highly gratified by more frequent opportunities of corresponding with the English College of Physicians on any subject which may conduce to the advancement of Science, and the welfare of the public.

I have the honour to be, Sir,

Your most obedient humble Servant,

JAMES HENTHORN, *Sec.*

At a meeting of the Royal College of Surgeons in Ireland, holden at their Theatre, on Tuesday the 13th day of January 1807.

FRANCIS M'EVoy, Esq. President.

Mr. Johnson reported from the committee, to whom was referred a letter from the College of Physicians, London, relative to the present state of Vaccination in the United Kingdom, &c. &c. That they met, and came to the following Resolutions:

That it appears to this Committee, That Inoculation with Vaccine Infection is now very generally adopted by the surgical practitioners in this part of the United Kingdom, as a preventive of Small-pox.

That it appears to this Committee, That from the 25th day of March 1800, to the 25th of November 1806, 11,504 persons have been inoculated with vaccine infection at the dispensary for infant poor, and 2,831 at the cow-pox institution, making a total of 14,335, exclusive of the number inoculated at hospitals and other places, where no registry is made and preserved.

That it is the opinion of this Committee, That the cow-pox has been found to be a mild disease, and rarely attended with danger, or any alarming symptom, and that the few cases of small-pox which have occurred in this country, after supposed vaccination, have been satisfactorily proved to have arisen from accidental circumstances, and cannot be attributed to the want of efficacy in the genuine vaccine infection as a preventive of small-pox.

That it is the opinion of this Committee, that the causes which have hitherto retarded the more general adoption of vaccination in Ireland, have, in a great measure, proceeded from the prejudices of the lower classes of the people, and the interest of some irregular practitioners.

To which Report the College agreed.\*

Extract from the Minutes.

JAMES HENTHORN, *Sec.*

\* In addition to the above highly satisfactory documents, it may be proper to state, that the English House of Commons, have just voted £20,000 additional to Dr. Jenner, as the Discoverer of Vaccination.—E.

*Christie, on Vaccination at Ceylon.*

THE number of inoculations in the vicinity of Columbo, has been greater in June than for many months past, in consequence of the appearance of small-pox, which broke out in the Bazar, and several other parts of this district in May last. I have not been able very distinctly to trace its origin, but as we have enjoyed an exemption from its influence here for nearly three years, and as it was imported from the coast to Aripo in February last, it seems most probable that the infection was brought from thence on the breaking up of the fishery in the end of April, more particularly as it is ascertained that patients were landed from Aripo with small-pox, both at Calpentya and Chilow.

Although the disease appeared in several parts of this district at the same time, I am happy to say, that by means of the precautions which have been taken, its propagation to any considerable extent has been prevented. As soon as the existence of small-pox in any village or street was ascertained, Vaccinators were immediately sent to the spot, to urge the people in the neighbourhood to submit to immediate inoculation, and when it was practicable, the patients infected with small-pox were removed to a secluded situation, near the Leper's Hospital, where every aid was afforded them.

I am happy to say, that for a great length of time there has been almost no prejudice against vaccination in this island, and that all the more intelligent inhabitants have the fullest confidence in it, as a certain preventive against small-pox; but as some futile attempts have lately been made in Europe, to shake the confidence of the public in the efficacy of this inestimable

discovery, particularly by inculcating an idea that the constitution is not permanently shielded by its influence, but that, at the end of three years, a fresh inoculation may perhaps be necessary, it must prove satisfactory and I hope convincing to mention, that amongst the many thousands who were inoculated in 1802, and the succeeding years, there is not a suspicion that any one of them has been affected with small-pox, though many of them have been exposed in various ways to the most virulent contagion.

The existence of small-pox at the present period, has enabled us to put some of our patients to the test after the expiration of nearly four years from the time of their vaccination. Mr. Morton accordingly, in May last, when acting as superintendant of vaccination in this district, inoculated with active variolous virus two patients, Katto, a slave girl, and Johannes Fernando, a Cingalese, who had passed through the cow-pox in Sep. 1802; and many others have, by my directions, been exposed to the infection, without any effect but slight local inflammation on the inoculated part.

I have also, with the same event, inoculated with and exposed to small-pox, several of the patients vaccinated last month, particularly Carolus Rhode and Jonan Tedoe, residing at Moolwal, with a view to prove, that our infection, though it has passed on this island alone through a regular succession of at least two hundred patients, without any renewal from the cow, still possesses, unimpaired, its genuine qualities and preventive virtues.

The following instance exemplifies in so striking a manner these facts, that I cannot resist the pleasure of relating it.—On the morning of the 11th instant, when visiting with Dr. High, staff surgeon, the small-pox patients near the Leper's Hospital, I was informed that a boy in one of the neighbour-

ing houses was affected with small-pox, which surpris'd me much, as I had been inform'd that, agreeably to my instructions, every person in the village, who had not had small-pox, had been vaccinated. We immediately repaired to the house, where we found a boy, named Gorkanize Savery, labouring under small-pox on the fourth day of the eruption. On inquiry, I found he had not been inoculated, because he and his friends said, that four years before he had pass'd through an eruptive disease, believed to have been the small-pox. The family consist'd of seven other persons, three of whom were aged, and had pass'd through the small-pox; two had been vaccinated in 1802, and the remaining two in May, 1806. The whole of them continued, by my direction, to live in the house with and attend on the boy with small-pox, who is now recovered, without having communicated the disease to any of them.

In a former letter I took occasion to mention, that, agreeably to the most certain information, at least one-third of all the inhabitants of this island, who were affected with natural small-pox, died. The proportion of deaths on the present occasion, justifies that calculation, as out of twenty patients who have been removed to the Hospital, and regularly treated, seven have died; and I have reason to believe, that the mortality amongst the patients, who remained in their own houses has been still greater.

The number of vaccinated patients, reported to me since the introduction of cow-pox in Ceylon in August, 1802, amounted on the 30th of June, 1806, to 47,523; and if, agreeably to a moderate calculation which I formerly made, not more than one-half of the inhabitants of this island escap'd natural small-pox, and of the half that had it, one-third died, we may, without over-rating the benefits of vaccination, fairly estimate, that of the 47,523 patients who have been inoculated in the



island, one-sixth of the whole, or 7920, would have otherwise died of the small-pox, which, previous to the introduction of vaccination, was almost every year epidemic at Columbo, and many other parts of the island.

I shall conclude with mentioning one fact, which may prove interesting to your medical readers, and, as far as I know, is new in the history of vaccination. It is, that Peter Baro, a boy of European parents, who has long been confined with confirmed leprosy, and whose features are much disfigured with that disease, was vaccinated on the 4th of June, 1806, and passed through the disease regularly.

It has with justice I think been supposed, that psora and other diseases of the skin diminished the susceptibility of cow-pox; and yet we find, in the present instance, a most inveterate disease did not prevent its taking effect, which ought to encourage us, in the event of the prevalence of small-pox, not to be deterred by the presence of other diseases, from giving vaccination a trial; for, had it not been practised with this boy, there is reason to apprehend that he might have fallen a martyr to small-pox, which prevailed in his vicinity.

Abstract of the Returns of Patients inoculated for the Vaccine Disease in different Districts of Ceylon, during the month of June, 1806.

DISTRICT.	Numb. Discharg- Inocu- ed with lated. Certific.		Failed.	Remain.
Columbo	1018	679	230	109
Caltura	111	108		3
Galle	248	88	1	159
Matura	213	122	27	64
Tangalle	200	143	10	47
Hambangtotte	60	26	4	30
Batticaloa	57	30	12	15
Trincomallie	41	21	6	14
Mullativo	36	25		11
Jaffna	276	214		62
Manar	117	65	14	38
Calpetyn	24	23	1	
Chilou	49	11	31	7
Negunibo	40	26	4	10
Total	2490	1581	340	569

Note. The proportion of failures is greater than usual in the Columbo district, in consequence of several people having been inoculated for the sake of security, who were uncertain as to their having had small-pox.

*Med. and Phys. Jour.*

*Jones, on Vaccination.**Owego Village, Aug. 4th, 1807.*

DEAR SIR,

Your's of April 6th 1806, inclosing vaccine matter, was duly received ; and I return my sincere thanks for the same. I succeeded with the matter sent, and vaccinated about one hundred and fifty in the spring and fall--it being so hard a matter to convince the people in this country of its utility, and so disagreeable to urge them repeatedly to receive the infection, that I relinquished the business entirely for that season.

Last May, the small-pox broke out the natural way, which struck terror through the village and its vicinity: they immediately applied to me to vaccinate them, but owing to their unbelief, I lost the infection. I was very fortunate however in obtaining it from Wilkesbarre in a few days, and the following week from New-York ; people were so much alarmed with the small-pox, that I was obliged to devote the most of my time to vaccinating for several weeks.

From the first small-pox patient, I inoculated six of my last years kine-pock patients, took them into the Hospital and punctured each in two places and inserted the fluid matter. With matter from the same patient, I afterwards inoculated four more that had the disease last year, and two that had had it five years before : every inoculation took, and increased, in some five, in some seven, and in others nine days ; at those periods a scab formed, and the inflammation subsided without producing indisposition in any. Several others that had had the kine-pock years before were inoculated by another person, with similar success.

These experiments had the desired effect and gave a rapid circulation to the vaccine disease, and in five weeks not a small-pox patient was to be heard of, notwithstanding my opponent in practice, strove to extend it.

As experience is necessary in ascertaining precisely the genuine cow-pock, you will excuse me for asking you a few questions on this subject, which your experience will enable you, readily to answer.

1st. Do you conceive the eighth day to be the *Sacred Boundary* (as Jenner says) when the areola has not commenced, and the fluid remains limpid?\*

2nd. Is the matter fit for use as long as it remains limpid, where the efflorescence *has* formed?

3rd. Is the person secure from the small-pox, that has a pock progressing regularly, as a pock, and has no symptoms of disease, nor forenefs or swelling in the axilla?†

4th. Is the success of the pock injured by taking matter from it once in considerable quantity, or frequently puncturing it?‡

5th. Does the scratching or bruising of the pock, so as to disfigure it in its early stages, injure it?§

\* The single circumstance that the vaccine scab is capable of communicating the genuine disease, is sufficient to eradicate an error, so dangerous to the preserving amongst us this invaluable blessing; for, if the scab contains the principle of infection, all the previously formed matter in the vesicle must necessarily contain it.—This circumstance involves also in the affirmative, the answer to the 2nd. Query.—*Ed.*

† As far as my experience and experiments reach, the absence of symptomatic disease, when the pock has progressed regularly, does not preclude the safety of the patient —It is not however certain in my mind, that some disorder of the system does not occur, although it may be too slight for observation.—*Ed.*

‡ My experience does not shew any hazard to exist in taking infection once in considerable quantity, or by frequently puncturing it, if due attention is paid in the operation.—*Ed.*

§ There is no doubt in my mind, that the scratching or irritating the pock in its early stage, so as to disfigure it, is a very fruitful cause of destruction to the salutary influence of the vaccine; as the new action excited in the part, too often makes its future progress only that of a spurious disease.—*Ed.*

I shall continue vaccinating through the summer: and in the autumn shall be under the necessity of inoculating several families for the small-pox, or throw business into an opponents hands,\* (both disagreeable) as they are determined not to risque the cow-pock—at the same time shall make many experiments on kine-pock patients, which makes me more inquisitive.

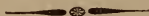
If there is any new treatise on this subject, that contains information, not to be found in that published by yourself, nor in Dr. Jenner's with plates, you will please to forward it, if convenient.—I have vaccinated more than all the rest of the physicians in this country, and wish to obtain all the information necessary, to prevent my patients being deceived.

Your obedient Servant,

WILLIAM JONES.

Dr. JOHN REDMAN COXE.

P. S. My kine-pock patients frequently break out with the disease, and sometimes have pock that progress regularly through all its stages, even when the indisposition has been trifling,—scarcely perceptible.



*Extract from RING on Vaccination.*

“IN the same work (the Madras Gazette) is a letter from Mr. Jukes, dated Bushire, March the 13th, 1805, giving an account of his introducing the practice into that part of the Persian dominions. This, he informs us, was done by means of equine matter, sent from Vienna; which produced a vesicle similar to the cow-pox, of the most distinct and regular kind.

\* “We see the right, and we approve it too  
Condemn the wrong, and yet the wrong pursue.”—*Ed.*

“From the hasty manner in which he travelled through the country, he could not expect to be very successful in diffusing the new inoculation ; and he moreover tells us, that they do not know the Persians, who suppose they have any notions of the public good ; or that any thing, except pleasure, or money, is capable of engrossing their attention.

“Mr. Jukes, however, at first met with some encouragement ; and began to indulge the pleasing hope of rapidly diffusing the practice through the whole empire. Numbers of people flocked to his house, in order to be inoculated ; and he had already begun to circulate pamphlets of instruction in the Persian language, as well as virus ; but the jealousy of the Shaikh was awakened, and he was obliged to desist, receiving only the most pointed insult, as his reward. The pretence for rejecting the blessing of vaccination was, that it had sprung from the impure hand of an unbeliever !

“This valuable publication of Dr. Anderson concludes with a letter from a native ; in which he expresses his grateful acknowledgements for the advantages which he had derived from vaccination, in his own family ; and declares, that since the operation, his children are much improved in their health.

“Four hundred and twenty-nine thousand, eight hundred and twenty-one, had been successfully vaccinated in the Presidency of Madras, and its dependencies, between the beginning of September, 1802, and the end of May, 1805, at the expense of 55,865 star-pagodas ; and two thousand, eight hundred and sixteen of them had been subsequently inoculated for the small-pox, which they all resisted.—This is a sufficient refutation of all the calumnies and falsehoods, which are daily circulated with so much industry, and effrontery, here at home.

I am, &c.

JOHN RING.”

*Med. and Phys. Jour.*

GENERAL ABSTRACT of Persons Vaccinated at the Presidency of Madras, and at the Subordinate Stations subject to the Authority of this Government, from the 1st of September, 1805, to the 31st of August, 1806.

DIVISIONS.	Medical Gentlemen superintending Vaccine Inoculation.	STATIONS.	CAST AND SEX OF PATIENTS DULY VACCINATED.						Total Vaccinated.		GRAND TOTAL.
			Christians.		Hindoos.		Mahomedans.		Males	Females	
			Males	Females	Males	Females	Males	Females			
NORTHERN.	{	Ganjam district - -	8	6	2054	1917	9	5	2171	1928	4099
		Vizagapatam district - -	87	124	1647	1377	143	150	1877	1651	3528
		Ingeram district - -	37	30	582	387	29	28	648	453	1103
		Rajahmundry district - -	236	178	413	2416	49	18	3698	2612	6310
		Masulipatam district - -	290	255	1541	1373	79	60	1910	1688	3598
CENTRE.	{	Guntur district - -	170	178	3783	3320	144	93	3927	3413	7340
		Vellore district - -	293	234	211	190	206	219	587	587	1174
		Nellore district - -	3460	2662	2329	2293	308	143	2930	2670	5600
		Prefidency and Jahgier - -	297	192	347	319	71	52	615	563	1178
		Trepafore district - -	640	423	2199	1581	705	421	3544	2425	5969
	{	Arnee district - -	401	355	628	624	111	74	1140	1043	2183
		Cuddalore district - -	62	104	714	905	19	22	807	1019	1826
		Ryacotah district - -	763	782	2428	2434	31	29	3222	3245	6467
		Salem district - -									



*The following Extract of a Letter from Dr. Moyan, dated, Pottsgrove, May 8th, 1806, seems alike to advance the Vaccine—and to demonstrate that the second Attack of Small-pox is not so rare as is usually supposed, or, that it is not so readily discriminated from Chicken-pox, as the Vaccine is from all other diseases.*

“AN event came to my knowledge last week which confirms the prophylactic nature of the Cow-pox as much I think, as a solitary instance could. Elizabeth Stincy was inoculated by Dr. Baum about sixteen years ago in Birdsborough. She was then between 3 and 4 years old. Her mother assured me that she had about 40 pustules of what appeared to be the small-pox. She now lives at the Trap married to Mr. Howard, and has a child sucking at the breast; she took the small-pox in the natural way and was once in consequence of it likely to die, but she now has got over it, and recruits her strength as is common for people after that malady. Her child now at her breast had been vaccinated and is not at all affected with the small-pox.

“The mother of this woman that has taken the small-pox, lately informed me that about eighteen more had taken the small pox, of those inoculated by Dr. Baum during the same winter; and also that they all had the small-pox very light when they were inoculated. Were they all inoculated with the Chicken-pox, instead of the small-pox? Dr. Smith, who attended the forementioned woman, and who lives at the Trap, I am told thinks so.

JOHN MOYAN.”



*Circular Queries of the London Medical Society, on the Influenza.*

*London, May 1803.*

SIR,

THE Medical Society of London, convinced that every practitioner will consider the late Epidemical disorder, commonly termed the Influenza, as an object peculiarly worthy of investigation, and wishing to collect for publication a complete history of the disease, earnestly request your answer to all or any of the subjoined questions.

1. Has any Epidemical Disorder appeared in your neighbourhood during the present spring, which differed from the usual diseases of the season?

2. On what day did it first shew itself?

3. When was it at its greatest height?

4. When did it disappear?

5. What were its symptoms, particularly the most urgent?

6. Did they vary much in different individuals? were they similar in members of the same family, in those of equal age, of similar constitutions, and of different sexes?

7. Has it been fatal when apparently unconnected with other diseases?

8. What proportion of fatality occurred?

9. What ages, classes, or constitutions, were most obnoxious to the disease, and felt it most severely or fatally?

10. Was the proportion of males or females greatest?
11. What did you find the best mode of treatment?
12. What effects followed bleeding, general or local, emetics, purgatives, opiates, sudorifics, blisters, or other remedies; and in what circumstances were they employed?
13. What was the proper diet?
14. What temperature was the most beneficial?
15. What was the usual mode of its termination?
16. Were relapses frequent?
17. Were the symptoms of relapses similar to the original attack, and were they more or less severe?
18. Did convalescents recover speedily?
19. In what state were they left by the disease?
20. What was the best treatment during convalescence?
21. What were the concomitant disorders which appeared to combine with the epidemic, and were they severe and fatal?
22. Has the present Influenza appeared to you to be contagious or not, and on what facts did you ground your opinion?
23. In Manufactories, Schools, Public Institutions, and other collections of people, how did it appear, and what was its progress?
24. Were a number of persons frequently affected in the same house, at the same period, or in succession; and at what intervals?

25. What are the sites, and other local circumstances of the places in which you have made your observations?

26. To what winds are they particularly exposed?

27. What places in your neighbourhood were affected with the disease previous to that in which you reside, and what immediately afterwards; and has the progress of the epidemic in these cases appeared to be in any degree regulated by intercourse?

28. What meteorological remarks have you made previously to the rise, and during the progress and decline of the epidemic?

29. Have you remarked whether the progress of the disease has in any degree followed the direction of the wind; and if this has appeared to be the case, can any deviations from such uniformity of progress be accounted for from any remarkable intercourse between different places?

30. Did you see any of the former Influenzas, and what are the analogies of the present with them?

31. Previously to, or during the prevalence of the disease, did you notice any epizootic complaint?

32. What other remarks have you made on this subject, which are not included under the present queries?

The Society being desirous to ascertain whether the epidemic be contagious or not, and to collect such meteorological observations as will throw light on the natural, as well as the medical history of the disease, particularly request your attention to these objects.

They do not mean to limit the answers of their Correspondents to their own observations, but will receive with pleasure any information relating to the above questions, which their corresponding members may have procured, and which they conceive authentic and valuable.\*

Signed by order of the Society.



### *Purification of Oil.*

M. Curaudeau has given two methods for purifying oil : the first is, “ to one hundred parts of rape-oil, one part of sulphuric acid is to be added, diluted with six times its weight of water, the mixture should be strongly agitated, and as soon as this is completely finished, it is left till the oil becomes clear, and when it is perfectly clear, the purification is effected,” The action of the sulphuric acid in this process, consists in depriving the oil of all its humidity, though it is itself mixed with water, and in separating from it a substance, the presence of which diminishes the energy of the combustion of the oil, covers the wick with charcoal, and produces much smoke ; on the abstraction of these principles, which are foreign to the oil, depends its quality of giving a good light. The second method is, “ to one hundred parts of rape-seed oil, ten parts of water are to be put, to which has been added one part of wheaten flour ; the mixture is to be well agitated, and then to

\* Though these Queries are dated so far back, it is presumed they may with propriety appear at this period, as a means of collecting information for publication in the Museum, from all parts of America, on the late extensive Epidemic ; and with this view the Editor solicits communications from his Medical Brethren of America.

be heated until all the water has been evaporated, or till the oil has ceased to have any union with the substances which it held in suspension," M. Curaudeau was led to this experiment from what is observable in the fauce called melted butter, which when too much boiled, is separated into the thick part that occupies the bottom of the vessel, while the other part is clear, and floats above. The lower substance is the caseous part of the butter united with the flower that has been added, and which the action of the fire has separated from the oil. The upper substance is the butter, deprived of all foreign matter, and is in fact purified butter.

*Monthly Review.*



*Toads inclosed under ground.*

At Headon, in Holderness, on the 6th of July 1804, three fine fat toads, and a large worm, covered up in an earthen pot, were buried two feet within the ground, and the earth rammed hard upon them. In June 1805, the toads were taken up, but the worm did not appear. The toads were in perfect health and strength, and in good spirits, though apparently somewhat reduced in size.

*Month. Mag.*



*Other Experiments on the Same.*

"In our paper of the 17th of January, we mentioned the circumstance of a toad having subsisted 15 months, con-

fined in a basin, and deposited at some depth below the surface of the earth, and that on the 8th of that month, it had been re-committed in like manner, to the ground, by Mr. John Walker of Bassenthwayte Chapple. On the 25th of last month, (July) his curiosity prompted him to see whether the reptile was still living; but upon uncovering the basin, there was nothing to be seen, but some bones, and a little slimy substance."

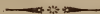
*Month. Mag.*



*Substitute for Salt of Sorrel.*

The following is said to be a good substitute for Salt of Sorrel, for removing ink spots and iron moulds. Take 6 parts of chrystals of tartar in powder; 3 parts of alum pulverised, and let them be mixed, and used in the same manner as Salt of Sorrel.

*Month. Mag.*



*Receipt for an excellent Glue.*

Fish glue three parts, well pounded and soaked in vinegar; common glue two parts, soaked in water; gum arabic one part; add a little sugar candy if you wish to have it sweet, (but of no other use) boil the whole together after having each ingredient separately soaked except the sugar, and when it has the consistence of common glue, pour it into a glazed earthen or china plate, previously rubbed with a little butter. When it is dried to the consistence of a thick jelly, then cut it in long pieces, and let it dry thoroughly in the sun for use.

*On the Period of Gestation in Animals.*

The exact period of gestation in different animals has not been ascertained; nor whether it is always the same, or liable to variations, in different individuals of the same species. The memoirs of the National Institute of France contain an interesting communication on this subject by M. *Teffier*, whose observations have led him to the following results.

## I. COWS.

One hundred and sixty cows were observed.

14 calved from the 241st to the 266th day; that is,  
from 8 months and 1 day, to 8 months and  
26 days.

3 — on the 270th day.

50 — from the 270th to the 280th day.

68 — from the 280th to the 290th day.

20 — on the 300th day.

5 — on the 308th day.

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Consequently there were 67 days between the two extremes.

## II. MARES.

One hundred and two mares were observed.

3 foaled on the 311th day.

1 — on the 314th day.

1 — on the 325th day.

1 — on the 326th day.

2 — on the 330th day.

47 — from the 340th to the 350th day.

25 — from the 350th to the 360th day.

21 — from the 360th to the 377th day.

1 — on the 394th day.

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This gives a latitude in the time of gestation of 83 days; and the following observation may be made respecting cows and mares, namely, that more of the first brought forth before the completion of the ninth month, than of the second before that of the eleventh.

### III. SOWS.

Of these only fifteen were observed.

- 1 brought forth young, which lived, on the 109th day;  
that is, 3 months and 19 days.
- 10 ——— from the 110th to the 120th day.
- 3 ——— on the 121st day.
- 1 ——— on the 122d day.
- 1 ——— on the 123d day.

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15

Consequently, the difference between the two extremes was 14 days.

### IV. RABBITS.

One hundred and thirty-nine were observed, during the course of three years.

- 1 brought forth on the 26th day.
- 2 ——— on the 27th day.
- 3 ——— on the 28th day.
- 53 ——— on the 29th day.
- 50 ——— on the 30th day.
- 21 ——— on the 31st day.
- 9 ——— on the 33d day.

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The difference between the two extremes on these animals was seven days.

*Med. and Chir. Rev.*



## CORRESPONDENCE.

*S. Carolina, January, 1807.*

SIR.

AS the London Medical and Physical Journal is in the hands of but few practitioners in this Country; the illiberal strictures of a Dr. Reid, in his "observations on the medical treatment of General Washington's last illness," may probably have never reached the eye of the respectable and experienced practitioners who attended that much lamented character. As those strictures have circulated in every place, to which the Med. and Phys. Journal has found admittance, it would seem but right to give the Gentlemen an opportunity to answer the remarks alluded to; which beyond doubt they are fully capable of doing, and of rescuing their names from the opprobrium which Dr. Reid has thrown upon them. With this view, I take the liberty of requesting you to give a place in your Museum to the report of his last illness, &c.; and to the observations which it has given rise to. Our country may consider it highly proper at a future day; and it cannot be done so effectually by any, as by those who attended at that period, nor at any time better than the present, when the circumstances must be yet alive within the recollection of the practitioners themselves.

I am Sir,

With respect,

Your well wisher,

DR. J. R. COXE.

URBANITAS.\*

\* The Editor must apologise for the delay of the present correspondence, from having unfortunately mislaid it, and having only within a short time discovered it. The pieces alluded to are introduced with much satisfaction, in hopes that the attack of Dr. Reid, on a point, which he could be but imperfectly master of, (as he in part admits) may meet with that candid notice it merits

From "THE TIMES," a Newspaper printed in Alexandria  
(Virginia), dated in December, 1799.

Messrs. J. & D. Westcott,

Prefuming that some account of the late illness and death of General WASHINGTON will be generally interesting, and particularly so to the professors and practitioners of medicine throughout America, we request you to publish the following statement.

JAMES CRAIK,  
ELISHA C. DICK.

SOME time in the night of Friday the 13<sup>th</sup> instant, having been exposed to rain on the preceding day, General Washington was attacked with an inflammatory affection of the upper part of the wind-pipe, called in technical language, *cynanche trachealis*. The disease commenced with a violent ague, accompanied with some pain in the upper and fore part of the throat, a sense of stricture in the same part, a cough, and a difficult rather than a painful deglutition, which were soon succeeded by fever and a quick and laborious respiration. The necessity of blood-letting suggesting itself to the General, he procured a bleeder in the neighbourhood, who took from his arm, in the night, twelve or fourteen ounces of blood: he would not by any means be prevailed upon by the family to send for the attending physician till the following morning, who arrived at Mount Vernon at about eleven o'clock on Saturday. Discovering the case to be highly alarming, and foreseeing the fatal tendency of the disease, two consulting physicians were immediately sent for, who arrived, one at half after three, the other at four o'clock in the afternoon. In the interim were employed two copious bleedings; a blister was applied to the part affected, two moderate doses of calomel were given, and an injection was administered, which operated on the lower

intestines—but all without any perceptible advantage ; the respiration becoming still more difficult and distressing.—Upon the arrival of the first of the consulting physicians, it was agreed, as there were yet no signs of accumulation in the bronchial vessels of the lungs, to try the result of another bleeding, when about thirty-two ounces of blood were drawn, without the smallest apparent alleviation of the disease. Vapours of vinegar and water were frequently inhaled, ten grains of calomel were given, succeeded by repeated doses of emetic tartar, amounting, in all, to five or six grains, with no other effect than a copious discharge from the bowels. The powers of life seemed now manifestly yielding to the force of the disorder. Blisters were applied to the extremities, together with a cataplasm of bran and vinegar to the throat. Speaking, which was painful from the beginning, now became almost impracticable ; respiration grew more and more contracted and imperfect, till half after eleven o'clock on Saturday night, when, retaining the full possession of his intellect, he expired without a struggle.

He was fully impressed at the beginning of his complaint, as well as through every succeeding stage of it, that its conclusion would be mortal, submitting to the several exertions made for his recovery rather as a duty than from any expectation of their efficacy. He considered the operations of death upon his system as coeval with the disease ; and several hours before his decease, after repeated efforts to be understood, succeeded in expressing a desire that he might be permitted to die without interruption.

During the short period of his illness he economized his time in the arrangement of such few concerns as required his attention, with the utmost serenity, and anticipated his ap-

proaching dissolution with every demonstration of that equanimity for which his whole life had been so uniformly and singularly conspicuous.

JAMES CRAIK,

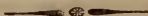
*Attending Physician.*

ELISHA C. DICK.

*Consulting Physician.*

The signature of Dr. Gustavus Brown, of Port-Tobacco, who attended as consulting physician, on account of the remoteness of his residence from the place, has not been procured to the foregoing statement.

*Med. Repository, Vol. 3d.*



*Observations on the Medical Treatment of General Washington's last Illness.* By JOHN REID, M. D. Physician of the Finsbury Dispensary, &c.

IN reading the official report of the death of General Washington, as stated in the news-papers, &c. I should imagine, there were few medical persons who did not feel astonishment at the very extraordinary manner in which that great man was treated by his physicians, during his last and fatal indisposition.

Some time in the night of the 13th of December, it is said, that the General was seized by a disease, called the Cynanche Trachealis.

During the same night, he sent for a bleeder, who took from him twelve or fourteen ounces of blood.

The next morning, a physician was sent for, who arrived at Mount Vernon, at eleven o'clock; when, imagining danger in the case, he advised the calling in of two consulting physicians.

In the interval, however, he thought proper to employ, in spite of the twelve or fourteen ounces that had already been expended, two copious bleedings. Now, when we consider that these are called copious, and the other is not noticed as such, and also the indifference with which a future most copious bleeding is afterwards mentioned, *we may presume*, that each of these was twenty-five, or twenty, ounces at least.

After this, "two moderate doses of calomel were administered." I know not exactly what a moderate American dose of calomel may be; but if, as it is fair to presume, it be in proportion to the bleedings, we may conclude, that it was at least very considerable.

Upon the arrival of the first of the consulting physicians, it was agreed, that as there were no signs of accumulation in the bronchial vessels of the lungs, they should try another bleeding.

Now this appears to be perfectly inexplicable.

As there were at present, no signs of accumulation in the bronchial vessels of the lungs; they were driven to another bleeding. Hence, it would seem, that this last bleeding was to *produce* an accumulation in the bronchial vessels of the lungs. There was great difficulty of breathing, great inflammation; but as there was, as yet, no accumulation in the lungs, they were determined to induce that also; and, as a likely mean of inducing it, had recourse to a most extravagant effusion of blood. This is not an unfair interpretation of their words; but it could not have been their real meaning: their real meaning it is impossible to discover. In addition to all the previous venesections, thirty-two ounces are now drawn! The medical

reader will not be surprised to find that this was unattended by any apparent alleviation of the disease.

In the next place, vapors of vinegar and water are frequently inhaled. Two doses of calomel were already given; but this is not deemed sufficient, ten grains of calomel are added: Nor is even this sufficient; repeated doses of emetic tartar, amounting, in all, to five or six grains, are next administered. It is said, "the powers of life now seemed to yield to the force of the disorder." To many it may appear, that the yielding of the vital principle, in these circumstances, was not *altogether* owing to the force of the disorder.

The patient, lying in this feeble and nearly exhausted state, is to be still farther tormented. Blisters are next applied to his extremities, together with a cataplasm of bran and vinegar to his throat.

It is observed, that "speaking, which was painful from the beginning, now became scarcely practicable." When we reflect upon that extreme degree of weakness to which the patient must, by this time, have been reduced, and that he had both a blister and a cataplasm of bran and vinegar at his throat, can we wonder that speaking would be scarcely practicable? Respiration grew more and more contracted and imperfect until after eleven o'clock on the Saturday night, when he expired without a struggle.

Think of a man being, within the brief space of little more than twelve hours, deprived of eighty or perhaps ninety ounces of blood; afterwards swallowing two moderate American doses of calomel, which were accompanied by an injection; then five grains of calomel, and five or six grains of emetic tartar; vapors of vinegar and water frequently inhaled; blisters applied to his extremities; a cataplasm of bran and vinegar to his throat, upon which a blister had been already affixed: Is it surprising that, when thus treated, the afflicted General, after various ineffectual struggles for utterance, at length articulated a desire that he might be allowed to die without interruption!

To have resisted the fatal operation of such Herculean remedies, one should imagine that this venerable old man ought at least to have retained the vigor of his earliest youth.

A British physician *may be deemed not competent to ascertain the propriety of trans-atlantic practice*; the current of blood, in the inhabitants of the new world, may bear some proportion to the current of its rivers; in that case, the medical treatment ought likewise to be conducted on a larger scale.

But this is a subject not proper for levity; it is a serious and solemn subject; and it is on that account that I have been induced to make the few preceding observations.

J. REID.

*London, April 21, 1800.*

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CORRESPONDENCE.

SIR,

HAVING had occasion, in consequence of the extraordinary commendations bestowed on a late work by Dr. Thomas Ewell, entitled "Plain Discourses on the laws and properties of Matter," &c. &c. in a review of it by the Editors of the New-York Medical Repository, to look at the work itself, under an impression of finding it possess superlative merit; I cannot but avow my extreme disappointment in perusing it; for so far from being an original production, the author does not hesitate to avow his obligations to a variety of authors; the quotations from whom, to all appearance form three-fourths of a bulky volume, of 6 or 700 pages octavo, and are the materials of chief value in the work. This mode of making a book, though very convenient to an author, is certainly not so to those purchasers, who already possess the original works from whence taken; and to whom it may be expedient to consult œconomy. The author, I am informed, is a very young man, who, within these two or three years, took his medical degree in the University of Pennsylvania; and as it is



probable his pursuits, in qualifying himself for that important point, fully occupied his time; it is by no means unfair to presume, that his own experience on the subjects of which he treats could not have been very extensive. This, though no argument against the writing of a book, would have been a good one in favour of delay in publication, until his own experience might have confirmed, or overthrown his own, or the opinions of others. I cannot, therefore, but wish he had laid it aside for revision, that time might have given him an opportunity of saying more from himself, and less from others. *Nōnum prematur in annum.*

But what has chiefly occasioned these remarks, is the late opportunity I have had of perusing a copious review of the same work in the third volume of the Boston Anthology; a review so totally unlike the former, that I am unable to account for two persons, setting down with the same object, regarding the work in so completely different a light; and, although I must confess my conviction of the justice of the latter, I would fain see them both reconciled.

This last named review is recommended to his candid attention, as it may prove useful to the future publications, which I perceive he has in contemplation.

PHILO.

*New Jersey, August, 1807.*



WE beg permission here, a second time, in conjunction with the Editors of the New York Medical Repository, to enter our protest, against the omission of the London Medical and Physical Journal, in noticing the sources of their selection, when taken from our American periodical works. In o. 100 of that work, no less than 15 pages are taken up with three cases from the Medical Museum, for which no acknowledgement is made—Nay! even some notes and modes of expression which might have discriminated the source, are entirely omitted. We are of opinion with the Editors of the Medical Repository, that “the rights of *meum* and *tuum* in literature and science, although it may not be possible to guard them so effectually as those of other kinds of property, are certainly entitled to regard”—and we trust the candour of the Editors of the London Medical and Physical Journal will influence their conduct in future.



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## MEDICAL AND PHILOSOPHICAL REGISTER.

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### FOREIGN AND DOMESTIC.

*Bardsley's experiments with the White Oxide of Bismuth.\**

DR. BARDSLEY next gives us the result of his experience with the *White Oxide of Bismuth*, as a remedy in certain dyspeptic complaints. His trials entirely confirm the character bestowed upon it by Dr. Marcet and others. As this medicine is only of late introduction, and its medical properties are as yet but little known, the testimony here given in its favour merits the attention of practitioners. It is proper to observe that the common *magistery of bismuth* commonly sold in the shops is very far from being in a pure state: a very pure white oxide of this metal is prepared and sold by Allen and Howard, of Lombard-street. The oxide of bismuth appears to be a mild as well as efficacious remedy; for in no instance has it been productive of any disagreeable effects. The following case affords a striking instance of the benefit that may be expected from this medicine.

“ Aug. 18, 1806. Hugh M'Guinnis, *æt.* 28, weaver. On account of the length and severity of this patient's disorder,

\* Medical reports of cases and experiments, with observations; chiefly derived from hospital practice, &c. &c. By S. A. Bardsley, M. D. &c.

he was admitted *into the Infirmary*. His chief complaint arises from a heavy pressing pain at his stomach, attended with a sense of constriction of the œsophagus, and eructations of an acid taste and flavour, chiefly after meals. Sometimes the matter brought up is so intensely acid, as to blister the palate and lips. He is frequently awakened in the night, with a spasmodic affection of the stomach, accompanied with a violent pulsation about the umbilical region. The animal functions are considerably deranged. He makes water with difficulty at times; and his bowels are alternately loose and costive. His appetite is uncertain, sometimes decayed, and sometimes greedy; but even its moderate gratification is sure to be attended with an increase of pain and sickness. His sleep is disturbed and unrefreshing, and his countenance betrays great mental anxiety, as well as bodily suffering. His complaint had existed for two years, and had greatly reduced the patient's bulk and strength. He had been accustomed to live chiefly on vegetable food of an acid and flatulent kind, and sometimes had indulged to excess in spirituous liquors. From the nature and duration of the disease, I was apprehensive there might be an organic affection of the stomach, and therefore my hopes of success were not sanguine. But as much uncertainty prevailed, and as my former experience had convinced me of the utility of bismuth in combating symptoms nearly allied to those which affected this patient, I resolved to give it a trial. He was accordingly ordered to take, after the bowels had been properly cleansed, half a drachm of the compound powder of bismuth, thrice a day, and to have a small blister applied to the region of the stomach. His diet to consist chiefly of animal food, and broth; and for common drink, a small portion of ardent spirit, plentifully diluted with water.

“ 24th. The patient has already obtained great relief. The stomach is able to sustain a moderate portion of food without

great uneasiness, and no longer rejects its contents in an acid state. The spasms are abated in frequency and duration, and the patient can now sleep uninterruptedly for several hours in the night. The pulsation at the navel continues troublesome, and the appetite is uniformly sickly, and languid. An infusion of bark with colombo was directed to be taken after each dose of the powders.

“ 27th. The patient's appetite and strength are much invigorated; but he is still harassed by the dull and pressing pain at his stomach, and sense of constriction at the œsophagus. A blister was ordered to be applied to the epigastric region, and his medicines to be continued.

“ Sept. 2d. The tone of the stomach is now completely restored, and, indeed, there is but little derangement in any of the animal functions. The bowels have become rather torpid, and require castor oil to promote their regular action. The medicines were persevered in till the 9th, when the patient was suddenly seized with an acute pain in the umbilical region, attended with an increase of the pulsatory sensation, which he has so often experienced in the same parts. The attack was preceded by rigors, and accompanied with a strong, full pulse, and feverish heat. I ordered about eight ounces of blood to be taken from the arm, the bowels, which were rather constipated, to be freely evacuated, and his medicines to be discontinued for the present. The paroxysm terminated in a few hours, and the patient was able the next day to resume his former plan, which he continued till the 15th, when he was discharged, completely cured.”

*Med. and Chirurg. Review.*

*Observations on Hydrophobia from the same.*

In his supplementary remarks, Dr. Bardsley contends, as he had before done, for the existence of *spontaneous hydrophobia* in the human subject, independent of all infection. He thinks, also, that the objections which some have made to the term *hydrophobia*, upon the supposition that a dread of water is not an essential symptom of the disease, are without foundation. There is scarcely, he observes, a single well authenticated history of the disease, in which an aversion to swallowing liquids was not perceived at some stage of the disorder, and therefore that the term *hydrophobia* (ὑδροφοβία) is at least as proper as either βραχυποσία or δυσκατάποσις, both of which imply a difficulty of swallowing, and more appropriately belong to topical organic affections of the organs of deglutition.

Respecting the origin of canine madness in animals, after questioning the validity of the opinions which refer it to certain remote causes, as putrid aliment, climate, deficiency of water, want of perspiration, and lastly, what is called the *worm under the tongue*, the author offers his own opinion, that the disease is never generated in animals but by contagion. In this idea he is supported by those whose extensive opportunities for observation entitle them to much consideration, namely, those gentlemen who are accustomed to keep hounds in packs for the purposes of the chase, and who must be admitted to be competent witnesses on a question of this nature.

“I have been favoured,” the author says, “with some important information, the result of experiment and actual observation, from a gentleman who appears to have investigated with an equal share of zeal and sagacity, the subject of rabies in dogs:—I allude to Walter Trevelyan, Esq. of Nether

Wilton near Morpeth, who, from his attachment to sporting, and his earnest wish to relieve mankind from the danger of this malady, has directed his endeavours both for its prevention and cure, among the canine species. I shall select from his correspondence such parts as throw light upon the general subject, and at the same time confirm some of the facts and arguments advanced in the course of this investigation. "I have kept hounds (he remarks) for many years, during which time, I have not had less than fifty couples of them go mad; not a single one could I cure by any medicine that was administered—The Ormskirk remedy was given in doses innumerable; worming under the tongue was also found useless. Sea bathing, large bleedings, and mercury, exhibited in various forms and quantities, were all tried in vain. The notion that the jaw of a mad dog, when wormed, (by drawing out a tendinous or muscular fibre from the under part of the tongue) will fall down and render the animal incapable of biting in consequence of the operation, is entirely erroneous; not but that the jaw of a dog, when mad, may drop down, and be incapable of biting; but by no means owing to the worming, for I have remarked the fact when no such operation has been performed.'

"Concerning the origin of the disease among his hounds he remarks, "The first occasion of madness among my pack arose from its being joined, when hunting, by a strange dog, who was soon perceived to be mad, but not before he had bitten many of the hounds. Within three weeks after, the symptoms broke out, when each hound was kept separate at such a distance as not to be able to bite each other. *They all died mad* at different intervals, according to their constitutions, or strength of the fever.' Again he observes, 'no pack when hunting can be said to be entirely safe, as frequently parts of it are out of sight in woods and covers; some of the hounds

are occasionally lost and absent for two or three days, and who can tell what may happen to them? Besides, how much mischief may occur at night from travelling mad dogs, which (as you say and I believe) will go to the distance of fifty miles from home?’

“In answer to a query concerning the length of time which takes place between the bite and the appearance of madness in dogs—Mr. Trevelyan states from his own observation, ‘That seven days may be considered a *fair average* of the shortest, and three weeks of the longest period from the date of the bite, to the occurrence of the disease, but this depends upon several circumstances: If a dog be confined, the fever is longer before it shews itself; but should the animal be at large, and hotly pursued, the irritation from an increased circulation of blood brings on the disease much sooner.’

“Mr. Trevelyan is firmly persuaded, contrary to the established opinion, that the canine virus is equally as contagious as the small-pox or measles; and he supports his opinion by the following statement of facts. ‘After losing my first pack (he remarks) I ordered all the straw to be taken out, the benches to be scalded with boiling water, and all the joints, cracks, &c. to be painted over, and filled up with hot diluted tar; the walls to be white-washed, the pavement thoroughly washed and cleaned with hot water. Being thus secure from infection (as I then thought) I collected another pack of hounds; yet madness occasionally broke out year after year. Thus kept in perpetual alarm, I ordered all the second pack to be destroyed. After having reasoned much within myself on the subject, I took up the idea, that the cause of the infection had not been entirely removed, notwithstanding my former precautions. I therefore ordered the pavement, in which the saliva, or other tainted excretions of the animals, might have

penetrated and lodged, to be taken up, together with all the earth in which it was bedded, and thrown into the river, and the kennel to be new painted, fumigated, white-washed, &c. and ever after the pack was free from infection. What still further strengthens my opinion of the subtle and contagious nature of the canine virus is the following fact: a game keeper, who lived at a distance (eight miles) assisted me daily in dissecting, &c. the hounds which died of madness. It happened once, that when he returned home, not having washed his hands after the operation, he had occasion to attend upon two bitches belonging to his master, that had whelps, which were confined in separate places half a mile distant from each other. When entering their respective kennels, with meat in his hands, they leaped up to smell at it, and instantly appeared disturbed, rubbing their noses among the grass, &c. Both these animals shortly went mad.\* The above statement is well worthy consideration; and although it do not afford decisive evidence of the capability of the canine virus, in the form of vapour, to produce infection; nor that the secretions and excretions of a mad dog are all alike poisonous, and may retain their infectious properties for a length of time; yet there is enough proven to excite attention to the facts, and at all events to demand the employment of the means pointed out for purifying kennels in which rabid dogs have been confined."

*Med. and Chirurg. Review.*

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*On the Anatomy, &c. of the Beaver.\**

The second chapter contains the anatomy of the beaver. After a general description of the animal, the author gives a minute and detailed account of the parts which furnish the castor. The four bags that are observed to be situated near

\* *Anatome castoris, atque chemica castorei analysis, ejusque in medicina usus.*—Auctore Andrea Conrado Bonn, Batav. &c. 1806.

the genital organs, and the two superior of which contain the castor, were taken by the antients for the testicles, and have since been the subject of various wild conjectures. These different opinions are here all brought forward.

The four bags mentioned, are formed by the external integuments, of which indeed they are properly continuations. The two inferior ones consist of a collection of a great number of small glandular bodies, that secrete a sort of unctuousity (*sebum* or *smegma*,) and are consequently true sebaceous glands, such as are found in various animals around the anus, and in other parts. The other two, or superior bags, are lined with a tunic, very like that of the gall-bladder, formed into a number of *rugæ*, which serve to increase its secretory surface. The fluid which this membrane secretes, and which is afterwards lodged in these sacs, is the proper castor, altogether different from the contents of the other two.

Both these matters are, in the author's opinion, of great utility in the economy of the animals, particularly in the act of coition. The first, he thinks, renders the parts more supple, and defends them from the water, in which this act is performed. The other, the castor, favours the venereal excitement (*æstrum venereum*,) by stimulating the penis of the male to erection, and by an equal action on the orifice of the genital parts in the female.

The third chapter is devoted to the analysis of castor, and the sort particularly selected by the author for the purpose was the Russian or Siberian. According to this analysis, castor consists of,

1. An ethereal oil, in quantity about one-third of the whole.
2. Adipocire, with a small portion of resin, about one-fourth.



3. Calcareous earth, one-fourth.

4. Cellular substance, one-sixth: this last part cannot properly be said to belong to the castor.

The more remote constituents of it are, oxygen, hydrogen, carbon, azote, calcareous earth, soda, phosphorus, and a small quantity of æther.

*Med. and Chir. Rev.*

PEARSON, *on the Eczema Mercuriale.*

The *Eczema Mercuriale* or mercurial rash is exceedingly well described. "When a man is about to be attacked by the Eczema, he sometimes complains of a heat and pruritus about the scrotum, and the upper and inner parts of the thighs, which, on examination, appear faintly red, and are somewhat rough. On other occasions, the heat, redness, and roughness, are first to be observed in the groins, and at the bend of the arms. In every case, which has come under my notice, the anterior parts of the body have been affected before the posterior parts, and the lower extremities have suffered from the Eczema, prior to its appearance on the trunk of the body: yet there is not so much of constancy and method in the order in which it proceeds, that the upper extremities are not occasionally attacked as early as the thighs, and I have seen each cheek considerably reddened and tumefied, before any part of the trunk was evidently affected. This redness, beginning on the extremities, makes a slow and gradual progress over the whole body, no part being exempted from it; its increase is attended with great tenderness of the skin, a troublesome itching, and an evident tumefaction of the parts affected; the swelling is not unlike that which attends the erysipelas, and I have seen it as

\* Observations on the effects of various articles of the *Materia Medica*, in the cure of Lues Venerica, &c. &c. By John Pearson, F. R. S. 2d. edit.

considerable as that which accompanies the confluent small-pox. The temperature of the skin is increased, the tongue is white, and the pulse frequent; but neither the functions of the stomach, nor of the sensorium commune, are evidently disturbed by this complaint.

“ The Eczema Mercuriale is always a vesicular disease, although the vesicles, which contain a pellucid fluid, are, at their first appearance, so small, that they cannot be easily distinguished from papulæ, without the aid of a convex glass; they are then seen to be distinct, each vesicle surrounded by a circle of redness, and, if they are not ruptured at an early period, they acquire the size of a large pin’s head, at which time their contents are opaque and puriform. The rupture of the vesicles is succeeded by a discharge of a thin acrid fluid, which seems to irritate the surface which it touches, and increases greatly the patient’s sufferings; and, as the disease proceeds, he is excoriated almost from head to foot. The quantity of discharge is in proportion to the extent of the excoriated surface; it is always considerable, and renders the linen, which absorbs it, stiff and unyielding. As the fluid discharged becomes thicker and more adhesive, it emits an offensive scent, similar to that which arises from the secretions of the sebaceous glands, when under the influence of disease.

“ As the Eczema Mercuriale does not invade the whole surface of the body at once, but occupies the different parts of it successively, so the several portions of the skin affected by it exhibit a more, or less, advanced state of the disease at the same time: hence, while the part first attacked is discharging the adhesive matter, the thin acrid fluid may be flowing from another portion. From this representation, it must be obvious that the exact period observed by this disease cannot be easily ascertained, nor, indeed, does it appear to be limited by any

regular term of duration. When the disease has affected but a small part of the body, I have seen it terminate in ten days; but, when it has been universal, the patient seldom recovers completely in less than six, eight, or ten weeks. When the discharge ceases, the loosened cuticle acquires first a pale brown colour, and then becomes nearly black, separating in large flakes, and leaving a faint redness on the exposed surface. This first desquamation is often succeeded by a second, or even a third; but in these latter desquamations the cuticle is more of a white colour, and separates in farinaceous-like scales, so that the surface of the skin appears as if it were covered with a white powder. The effects of the *Eczema Mercuriale* are not, however, confined to the destruction of the epidermis. I have known all the hair of the body, the beard, the hair under the axilla, and on the regio pubis, and the greater part of the eye-brows and hair of the head, separate, and leave the parts as smooth as in a state of infancy; but the eye-lashes do not usually fall off in this disease, although there is generally a redness about the tarfi and inner covering of the eye-lids. In one case, the nails of the fingers and toes separated from their attachment, and came away, and were succeeded by others of an irregular and deformed appearance, not unlike the nails of persons afflicted with lepra."

With regard to the treatment of this troublesome affection, Mr. Pearson doubts whether any remedies have the power of interrupting its regular course, or of abridging its duration. It ceases, however, spontaneously after a time, and has never proved fatal. The irritation may be relieved by a cooling regimen, and the application of a saturnine ointment to the excoriated parts.

*Med. and Chir. Rev.*

*On the Chemical Re-action of Medicines on each other.*

It is perhaps not generally known, that nitric æther is so readily decomposed as is actually the case; a fact of some consequence to be known to prescribers, and which shews the necessity of a minute acquaintance with the chemical agencies of different bodies on one another. A physician had occasion to prescribe nitric æther in combination with some distilled waters, and the syrup of poppies. The patient, who took the draught thus compounded, complained, to the surprise of his physician, of its acid taste and offensive odour.—The fact is, that nitric æther, when in contact with water, and especially if the water has sugar dissolved in it, is decomposed, and nitrous acid and nitrous gas are formed. It is even decomposed spontaneously by keeping, when exposed to a temperature below 60° F.; while, at a temperature above 88° F. it enters into ebullition, and bursts the vessels that contain it.

It follows, that when nitric æther is prescribed, it should be kept separate in a closely stopped phial, at a temperature at or below 60 F.; and the quantity to be taken should be poured out, and mixed with the intended vehicle, at the moment of using it.

*Med. and Chir. Rev.*

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*Mode of preserving Vegetable Juices and Fruits.*

It has long been a common practice in wine countries, to fumigate with sulphur the vessels intended for keeping wine. M. Cadet, an apothecary of Paris, applies this means of preservation to the depurated juices of plants. He has ascertained, he says, by numerous experiments, that this mode is prefer-

able to all those that have been employed hitherto; such as the exclusion of air, a reduced temperature, and the addition of alcohol. He has by him the juices of quinces and barberies, which, after three years keeping in fumigated bottles, are as fresh, he says, and in as good preservation, as when first expressed. The operation is exceedingly simple. It consists merely in introducing a lighted match with brimstone into the inverted bottle for an instant, withdrawing it again as soon as the bottle appears full of the vapour. The liquor is then immediately to be put into the bottle, which is to be closely stopped, and deposited in a cool situation. This mode appears to prevent effectually the fermentation of the juice, without imparting to it any unpleasant smell or flavour.

*Med. and Chir. Rev.*

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*Calculus Discharged by Abscess.*

A case has been lately published by M. Caumont, in the *Recueil Periodique de la Société de Médecine*, of an urinary calculus discharged through an abscess which formed in the hypogastric region. The stone weighed no less than nine drachms.

*Med. and Chir. Rev.*

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*Of the Acid contained in Urine.*

It has been generally supposed hitherto, that the acid which exists constantly in the free state in human urine, was the phosphoric acid. This opinion rested principally upon the urine being found to contain a considerable quantity of phosphate of lime, which, being insoluble in the perfectly neutralized state,

becomes exceedingly soluble, and even deliquescent, with an excess of acid. M. *Thenard* has lately read before the *French National Institute* a memoir, in which he proves that the disengaged acid in human urine is the acetous, an acid which is found equally in the sweat and in the milk.

*Med. and Chir. Rev.*

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*Singular Instance of Bulimia.*

There is at present, in St. Bartholomew's Hospital, a young woman, twenty-five years of age, named Sarah Brooks, who takes an uncommon quantity of food, but under very singular circumstances. She sleeps very little, scarcely half an hour in the space of twenty-four hours. At all other times she is incessantly eating. She consumes regularly two quartern loaves a-day, weighing nearly nine pounds; and twice a-day she eats a pound and a half of beef steaks. She drinks about three pints of porter daily. Yet she declares she has no appetite, but is impelled to eat by an intolerable sinking sensation at the stomach, which is only relieved by continually swallowing food. As a proof that she has none of the usual inclination for food, she can eat only bread and beef. All other kind of food is disagreeable to her. Even beef dressed in another manner would be disliked. She has a strong desire to drink cold water, but abhors the taste of spirits. She has had two children. The last time she lay in was seven months ago. She suckled her child for two months, when she weaned it on account of the weak state of her health. From this time her present complaint has been gradually coming on, and has been nearly in its present state for the last two months. She has a constant pain in the right side of the abdomen, which is considerably but equally enlarged, and without any particular

hardness or fluctuation. She has a constant diarrhœa. Her urine is in natural quantity. She feels always thirsty, and has an universal sensation of a burning heat, though her skin is not remarkably hot to the touch, except in the palms of the hands. Her uneasy feelings altogether, particularly that of sinking at the stomach, are so distressing, that she says she is weary of her life. She is of a full habit of body: her countenance and eyes are natural, and she has no feeling of pain in the head. She feels great relief from washing her hands, feet, and neck with cold water, and is highly gratified by being suffered to have a basin of cold water by her, to dip her hands into frequently, and to sprinkle herself with. Before the present attack she was a very little eater. Her menses are regular. A variety of medicines have been employed, but hitherto, it seems, without advantage. *Med. and Chir. Rev.*

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*An Account of two Children born with Cataracts in their Eyes, to shew that their Sight was obscured in very different Degrees; with Experiments to determine the proportional Knowledge of Objects acquired by them immediately after the Cataracts were removed.\**

“ Mr. Cheselden’s observations on this subject, recorded in the Philosophical Transactions for the year 1728, pointed out two material facts; that vision alone gives no idea of the figure of objects, or their distance from the eye, since a very intelligent boy, thirteen years of age, upon recovering his sight was unable to distinguish the outline of any thing placed before him, and thought that every object touched his eye.

\* Philosophical Transactions of the Royal Society for 1807..

“ Mr. Ware’s cases, which have also a place in the *Philosophical Transactions* for 1801, and are compared with that of Mr. Chefelden, appear to lead to a different conclusion. The following observations are laid before the society with a view to explain this circumstance.

#### CASE 1.

“ William Stiff, 12 years of age, was admitted into St. George’s Hospital under my care on the 17th of July 1806, with cataracts in his eyes, which, according to the account of his mother, existed at the time of his birth. From earliest infancy he never stretched out his hand to catch at any thing, nor were his eyes directed to objects placed before him, but rolled about in a very unusual manner, although in other respects he was a lively child. The eyes were not examined till he was six months old, and at that time the cataracts were as distinct as when he was received into the hospital.

“ Previous to an operation being performed, the following circumstances were ascertained respecting his vision. He could distinguish light from darkness, and the light of the sun from that of a fire or candle: he said it was redder, and more pleasant to look at, but lightning made a still stronger impression on his eyes. All these different lights he called red. The sun appeared to him the size of his hat. The candle flame was larger than his finger, and smaller than his arm. When he looked at the sun, he said it appeared to touch his eye. When a lighted candle was placed before him, both his eyes were directed towards it, and moved together. When it was at any nearer distance than twelve inches, he said it touched his eyes. When moved further off, he said it did not touch them; and and at twenty-two inches it became invisible.



“On the 21st of July, the operation of extracting the crystalline lens was performed on the left eye. The capsule of the lens was so very strong as to require some force to penetrate it. When wounded, the contents, which were fluid, rushed out with great violence. Light became very distressing to his eye, and gave him pain. After allowing the eye-lids to remain closed for a few minutes, and then opening them, the pupil appeared clear, but he could not bear exposure to light. On my asking him what he had seen, he said, ‘your head, which seemed to touch my eye:’ but he could not tell its shape. He went to bed, and took an opiate draught: the pain in his eye lasted about an hour, after which he fell asleep. The whole of that day the light was distressing to his eye, so that he could not bear the least exposure to it.

“On the 22d the eye-lids were opened to examine the eye. The light was less offensive. He said he saw my head, which touched his eye. There was so much inflammation on the eye-ball, that a leech was applied to the temple, and the common means for removing inflammation were used.

“On the 23d the eye was less inflamed, and he could bear a weak light. The pupil was of an irregular figure, and the wounded cornea had not united with a smooth surface. He said he could see several gentlemen round him, but could not describe their figure. My face, while I was looking at his eye, he said was round and red.

“On the 25th the inflammation had subsided, but on the 27th returned, and continued notwithstanding different means were employed for its removal, till the first of August, when it was almost entirely gone. On the fourth the eye was appa-

rently so well, that an attempt was made in the presence of Mr. Cavendish and Dr. Wollaston to ascertain its powers of vision; but it was so weak that it became necessary to shade the glare of light by hanging a white cloth before the window. The least exertion fatigued the eye, and the cicatrix on the cornea, to which the iris had become attached, drew it down so as considerably to diminish the pupil. From these circumstances nothing could be satisfactorily made out respecting the boy's vision. On the 11th a second attempt was made in the presence of Mr. Cavendish, but the pupil continued so contracted and irregular, and the eye so imperfect in its powers, that it became necessary a second time to postpone any experiments.

“ On the 16th of September the right eye was couched. This operation was preferred after what had happened to the other eye, in the hope that there would not be the same degree of inflammation; and as the former cataract was fluid, there was every reason to believe that couching would in this instance be most efficacious.

“ The operation gave pain, and the light was so distressing to his eye that the lids were closed as soon as it was over, and he was put to bed. The consequent inflammation was not severe, but as soon as the fluid cataract which had been diffused through the aqueous humour was absorbed, the capsule of the lens was found to be opaque, and the sight consequently imperfect. The eyes were not examined with respect to their vision till the 13th of October, during which period the boy remained quiet in the hospital. On that day the upper part of the pupil of the left eye had in some measure recovered its natural state, and had become transparent, but the cicatrix in the cornea was more extensively opaque than before. The light now was not distressing to either eye, and when strong, he could

readily discern a white, red, or yellow colour, particularly when bright and shining. The sun and other objects did not now seem to touch his eyes as before; they appeared to be at a short distance from him. The eye which had been couched had the most distinct vision of the two, but in both it was imperfect. The distance at which he saw best was five inches.

“When the object was of a bright colour, and illuminated by a strong light, he could make out that it was flat and broad; and when one corner of a square substance was pointed out to him, he saw it, and could find out the other, which was at the end of the same side, but could not do this under less favourable circumstances. When the four corners of a white card were pointed out, and he had examined them, he seemed to know them: but when the opposite surface of the same card, which was yellow, was placed before him, he could not tell whether it had corners or not, so that he had not acquired any correct knowledge of them, since he could not apply it to the next coloured surface, whose form was exactly the same with that, the outline of which the eye had just been taught to trace.

## CASE 2.

“John Salter, seven years of age, was admitted into St. George’s hospital on the first of October, 1806, under my care, with cataracts in both eyes, which according to the accounts of his relations had existed from his birth.

“After he was received into the hospital, the following circumstances were ascertained respecting his vision. The pupils

contracted considerably when a lighted candle was placed before him, and dilated as soon as it was withdrawn. He was capable of distinguishing colours with tolerable accuracy, particularly the more bright and vivid ones.

“On the sixth of October the left eye was couched. This operation was preferred to extraction, from a belief that the cataracts were not solid, and as the injury done to the capsule by the operation would be less, there was not the same chance of inflammation, the disposition for which had been so strong in the former case. As the eye was not irritable, and was likely to be but little disturbed by this operation, every thing was previously got ready for ascertaining his knowledge of objects, as soon as the operation was over, should the circumstances prove favourable. The operation was attended with success, and gave very little pain. The eye was allowed ten minutes to recover itself: a round piece of card of a yellow colour, one inch in diameter, was then placed about six inches from it. He said immediately that it was yellow and on being asked its shape, said, ‘Let me touch it, and I will tell you.’ Being told that he must not touch it, after looking for some time, he said it was round. A square blue card, nearly the same size, being put before him, he said it was blue and round. A triangular piece he also called round. The different colours of the objects placed before him he instantly decided on with great correctness, but had no idea of their form. He moved his eye to different distances, and seemed to see best at 6 or 7 inches. His focal distance has been since ascertained to be 7 inches. He was asked whether the object seemed to touch his eye, he said, ‘No;’ but when desired to say at what distance it was, he could not tell. These experiments were made in the theatre of the hospital, in which the operation was performed, before the surgeons and all the students. He was highly delighted

with the pleasure of seeing, and said it was 'so pretty,' even when no object was before him, only the light upon his eye. The eye was covered, and he was put to bed, and told to keep himself quiet, but upon the house-surgeon going to him half an hour afterwards, his eye was found uncovered, and he was looking at his bed curtains, which were close drawn. The bandage was replaced, but so delighted was the boy with seeing, that he again immediately removed it. This circumstance distressed the house-surgeon, who had been directed to prevent him from looking at any thing till the next day when the experiment was to be repeated. Finding that he could not enforce his instructions, he thought it most adviseable to repeat the experiment about two hours after the operation. At first the boy called the different cards round ; but upon being shewn a square, and asked if he could find any corners to it, he was very desirous of touching it. This being refused, he examined it for some time, and said at last that he had found a corner, and then readily counted the four corners of the square ; and afterwards, when a triangle was shewn him, he counted the corners in the same way ; but in doing so his eye went along the edge from corner to corner, naming them as he went along.

"Next day, when I saw him, he told me he had seen 'the soldiers with their fifes and pretty things.' The guards in the morning had marched past the hospital with their band ; on hearing the music he had got out of bed, and gone to the window to look at them. Seeing the bright barrels of the muskets, he must in his mind have connected them with the sounds which he heard, and mistaken them for musical instruments. On examining the eye 24 hours after the operation, the pupil was found to be clear. A pair of scissors was shewn him, and he said it was a knife. On being told he was wrong, he could not make them out ; but the moment he touched them he said

they were scissors and seemed delighted with the discovery. On being shewn a guinea at the distance of 15 inches from his eye, he said it was a seven shilling piece; but placing it about 5 inches from his eye, he knew it to be a guinea; and made the same mistake as often as the experiment was repeated.

“From this time he was constantly improving himself by looking at, and examining with his hands, every thing within his reach, but he frequently forgot what he had learnt. On the 10th I saw him again, and I told him his eye was so well that he might go about as he pleased without leaving the room. He immediately went to the window, and called out, ‘What is that moving?’ I asked him what he thought it was? He said, ‘A dog drawing a wheel-barrow. There is one, two, three dogs, drawing another. How very pretty!’ These proved to be carts and horses on the road, which he saw from a two pair of stairs window.

“On the 19th, the different coloured pieces of card were separately placed before his eye, and so little had he gained in thirteen days, that he could not without counting their corners one by one tell their shape. This he did with great facility, running his eye quickly along the outline, so that it was evident he was still learning, just as a child learns to read. He had got so far as to know the angles, when they were placed before him, and to count the number belonging to any one object.

“The reason of his making so slow a progress was, that these figures had never been subjected to examination by touch, and were unlike any thing he was accustomed to see.

“He had got so much the habit of assisting his eyes with his hands, that nothing but holding them could keep them from the object.

“On the 26th the experiments were again repeated on the couched eye, to ascertain the degree of improvement which had been made. It was now found that the boy, on looking at any one of the cards in a good light, could tell the form nearly as readily as the colour.

“From these two cases the following conclusions may be drawn :

“That, where the eye before the cataract is removed, has only been capable of discerning light, without being able to distinguish colours; objects after its removal will seem to touch the eye, and there will be no knowledge of their outline; which confirms the observations made by Mr. Cheselden.

“That where the eye has previously distinguished colours, there must also be an imperfect knowledge of distances, but not of outline, which however will afterwards be very soon acquired, as happened in Mr. Ware’s cases. This is proved by the history of the first boy in the present paper, who before the operation had no knowledge of colours or distances, but after it, when his eye had only arrived at the same state, that the second boy’s was in before the operation, he had learnt that the objects were at a distance, and of different colours : that when a child has acquired a new sense, nothing but great pain or absolute coercion will prevent him from making use of it.

“ In a practical view, these cases confirm every thing that has been stated by Mr. Pott and Mr. Ware, in proof of cataracts in children being generally soft, and in favour of couching, as being the operation best adapted for removing them. They also lead us to a conclusion of no small importance, which has not before been adverted to; that when the cataract has assumed a fluid form, the capsule, which is naturally a thin transparent membrane, has to resist the pressure of this fluid, which like every other diseased accumulation is liable to increase, and distend it, and therefore the capsule is rendered thicker and more opaque in its substance, like the coats of encysted tumours in general.

“ As such a change is liable to take place, the earlier the operation is performed in all children, who have cataracts completely formed, the greater is their chance of having distinct vision after the operation. It is unnecessary to point out the advantages to be derived from its being done at a more early age, independent of those respecting the operation itself.”

*Med. and Chir. Rev.*

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*Observations, on the Structure of the different Cavities which constitute the Stomach of the Whale, compared with those of ruminating Animals, with a view to ascertain the Situation of the Digestive Organ; by the same.\**

“ The following observations,” Mr. Home remarks, “ are in some measure a continuation of those upon the stomachs of

\* From the Philosophical Transactions, for 1807. The author Mr. Home, (omitted in the preceding paper, by mistake).



ruminating animals, contained in a former paper.\* They are intended to shew that the stomach of the whale forms a link in the gradation towards the stomachs of truly carnivorous animals.

“This subject was brought under my consideration by the following circumstances. While at Worthing on the Suffex coast, in the month of August last, a *Delphinus Delphis* of Linnaeus, or small bottle-nose whale of Mr. Hunter, was brought on shore by the fishermen alive. I immediately purchased it, with a view of enriching the Hunterian collection with the skeleton, and other parts of its structure.

“The stomach was the particular object of my own attention; for, having been so lately employed in considering the stomachs of ruminating animals, I was pleased with an opportunity of examining in a recent state the stomach of one of the whale tribe, to which the porpoise belongs, with a view to ascertain more accurately than had been hitherto done, the real resemblance between its structure and that of the stomachs of ruminating animals.

“The structure of the stomach of one species of whale was not new to me, having twenty years ago assisted Mr. Hunter in dissecting the piked whale, but at that time I only viewed the different parts of its structure with the eye of a common observer, while now my mind was particularly directed to the peculiarities of the stomach. In this examination I discovered a resemblance between the second, third, and fourth cavities in the whale, and the two portions of the fourth cavity in the

\* See p. lxxv of this vol. of the Museum.

bullock and camel, which appears to throw some light upon the uses of those parts, as well as upon digestion in general.

“As in the former paper a particular description was given of the stomach of the bullock and camel, as examples of ruminants with and without horns, it will be proper here to describe the stomach of the bottle-nose porpoise, as an example of the whale tribe.

“In the bottle-nose porpoise the œsophagus is very wide, has a number of longitudinal folds, and is lined with a strong white cuticle, which is continued over the internal surface of the first stomach.

“The first stomach lies in the direction of the œsophagus, which is continued into it, there being no contraction to mark its origin. It is of an oval form, and bears a strong resemblance in shape to a Florence flask. The cavity is 15 inches in length, and 9 in diameter. The internal surface has a very corrugated appearance, and its cuticular covering is thick and strong. The coats of the cavity are firm, and its bottom is surrounded by a strong muscular covering.

“The orifice which leads to the second stomach is at right angles to the cavity, and is situated a little way below the termination of the œsophagus. It is surrounded by several semicircular doublings of the internal membrane: the broadest of these is on the lower part; these are thick, and appear to be glandular.

“There is a canal between the first and second cavities three inches long, which opens into the second by a projecting orifice,

and the cuticular covering of the first stomach terminates immediately beyond this orifice, which is two and an half inches in diameter.

“This second stomach is nearly spherical, about seven inches in diameter. Its internal surface has a honey-combed appearance, formed by soft ridges of a glandular structure, leaving interstices of some depth between them. This structure gives the coats a considerable degree of thickness.

“The opening into the third stomach is almost close to that which enters the second, and is only  $\frac{5}{8}$  of an inch in diameter.

“The third cavity is nearly spherical, and is two inches in diameter. Its internal surface is smooth, and there are every where small orifices of ducts of glands opening into its cavity. The aperture which communicates between this and the fourth stomach is  $\frac{3}{8}$  of an inch in diameter.

“The fourth cavity is nearly cylindrical like an intestine, but rather widest at its furthest extremity. It is  $14\frac{1}{2}$  inches long; its greatest diameter is three inches. The internal membrane is smooth, and for three inches towards its origin and four inches towards its termination has numerous orifices through which secretions are poured into the cavity. The pylorus, which is the boundary of this stomach, is a round orifice  $\frac{2}{9}$  of an inch in diameter.

“Immediately beyond the pylorus there is a dilatation of the gut, which both Cuvier and Hunter call a cavity belonging to the stomach. It must however be considered as duodenum, since the common duct of the liver and pancreas opens into it; the longitudinal *valvula conniventes* have their origin in it; and

there is no transverse constriction any where beyond it, to mark the beginning of an intestine. Such an enlargement of the duodenum is very common in other animals, and has been described in the account of the camel. The coats of this portion of the duodenum are thicker than those of the fourth stomach.

“The number of cavities constituting the stomach are not the same in all animals of the whale tribe. In the common porpoise, grampus, and piked whale, the number is the same as in the bottle-nose porpoise; but in the bottle-nose whale of Dale there are two more cavities. This variation is however by no means material, since the general structure of the stomach is the same.

“In all of the whale tribe there is one cavity lined with a cuticle, as in the bullock and camel.

“In all of them there is a second cavity made up of a very glandular structure. In the porpoise, grampus, and large bottle-nose whale, this structure resembles that which is above described. In the piked whale the rugæ are longitudinal and deep, but in some places united by cross bands; and as the piked whale has whalebone teeth, the great whalebone whale will probably, from the analogy of its teeth, resemble it in the structure of its stomach.

“The third cavity in all of them is very small, and bears a strong resemblance to the third cavity in the camel’s stomach; its use, therefore, is probably the same.

“The fourth stomach in all of them has a smooth internal surface, with the orifices of glands opening into its cavity. In the bottle-nose whale of Dale the two additional cavities have the same internal structure, and therefore must have the same

general use, with a greater extension of surface, and the subdivisions will make the food pass more slowly into the intestine.

“The first stomach of the whale is not only a reservoir, but the food undergoes a considerable change in it. The flesh is entirely separated from the bones in this cavity, which proves that the secretion from the glandular part has a solvent power. This was found to be the case in the bottle-nose porpoise and large bottle-nose whale. In both of them several handfuls of bones were found in the first stomach without the smallest remains of the fish to which they belonged. The soft parts only can be conveyed into the second and third stomachs, the orifices being too small to admit the bones to pass.

“The bones must therefore be reduced to a jelly in the first stomach, and although the process, by which this is effected, being slower than that which separates the flesh, is the reason of their being found in such quantity in the cavity, the means by which it is performed are probably the same.

“The second cavity was supposed by Mr. Hunter to be the true digesting stomach, in which the food becomes chyle, and the use of the third and fourth he looked upon as not exactly ascertained.\*

“Upon what ground Mr. Hunter was led to draw this conclusion cannot now be ascertained; and, such is my respect for his opinion, that nothing but the following observations, supported by facts, could lead me to form a different one. In

\* Vide, *Observations on the Structure and Economy of Whales.* By John Hunter. *Phil. Transf.* vol. lxxvii. p. 411.

considering this subject, it struck me that the second stomach could not be that, in which chyle is formed, since that process having been completed, any other cavities would be superfluous. The last cavity in all stomachs is that, in which the process must be brought to perfection; and therefore the most essential change, which the food undergoes, or that by which it is formed into chyle, should be performed in that cavity. Surveying the different cavities in the whale's and ruminating stomachs with this impression on my mind, and comparing them with the single stomachs of carnivorous animals, it appeared that the first point, which required to be ascertained was, which of the cavities in these more complex stomachs bears the greatest resemblance to the simple one. The fourth of the whale is certainly more like the human stomach than the second or third. I therefore concluded that the fourth, both from analogy and situation, is the stomach in which the process is completed: and that in this animal, from the peculiarities of its œconomy, and the nature of the food, not only a cuticular stomach is necessary, but also two glandular ones, in which it undergoes changes preparatory to its being converted into chyle.

“Having satisfied myself upon this subject, and having compared the stomachs of the whale, with the fourth of the camel, the contraction or partial division of the camels, made it apparent that the lower portion only of that cavity, which resembles in shape and internal appearance the human stomach, is the cavity in which chyle is formed, and the upper or pliated portion is only to prepare the food, and is therefore analogous to the second in the whale.

“As the same appearances are met with in the fourth stomach of the bullock, as well as in the camel, although there is no

permanent contraction or division between them, the upper or plicated portion must be considered as a preparatory organ, and the lower portion as that, in which the formation of chyle is completed. This receives further confirmation from a more attentive examination of the parts, immediately after death, by which it was found that before the stomach has been disturbed there is an evident muscular contraction between the plicated and lower portion. This appearance was met with in every instance that was examined, and these were not fewer than nine or ten. Added to this the lower portion, on a more minute inspection, has an appearance somewhat similar to the inner membrane of the human stomach: and the surface of the plicæ is in many respects different.

“From the facts and observations which have been stated, it appears that in many animals of the class Mammalia, the food undergoes different changes preparatory to its being converted into chyle, and this last process is effected by a somewhat similar secretion, since the part of the stomach which produces it has in all of them an evident similarity of structure.

“The above facts appear to throw some light on the digestion of the different kinds of food, and open a wide field of inquiry into one of the most interesting parts of the animal economy which has been hitherto too much neglected. In the present very limited state of our knowledge there are many circumstances which cannot be accounted for: these, however, will be explained when a further progress has been made in this investigation.

“It is obvious, that as the stomachs of carnivorous animals are the most simple, animal substances, on which they feed, require a shorter process to convert them into chyle than vegeta-

bles ; but why the whale tribe, which live on fish, should have a more complex stomach, it is not easy to explain : since fish are very readily converted into chyle, in the stomachs of animals of their own class, as well as in the human stomach, and there is therefore reason to believe that they require as little preparation for that process, if not less than animal substances.

“ The fish bones swallowed by the whale tribe being retained in the cuticular bag, till they are reduced to jelly, explains the circumstance of cows and other ruminating animals being able occasionally to live on fish, (a fact, of which there is no doubt, both in the Orkneys and in Iceland,) since, if the bones are dissolved in the paunch, the other stomachs are in no danger of being injured from the animal living on this kind of food.

Whether these cavities, which I have called preparatory stomachs, are solely for purposes connected with digestion, or are also in any way connected with the formation of secretions peculiar to those animals, cannot be ascertained in the present state of our knowledge of digestion.

“ The oil of the physeter, which crystallizes into spermaceti, shews some affinity in this respect to the secretion of fat that becomes suet, which is only met with in ruminating animals ; but on the other hand, the oil of the rest of the whale tribe does not form this substance, more than the fat of the horse produces tallow. These facts may be afterwards explained by an examination of the digestive organs of the physeter, when an anatomist shall have an opportunity of examining them.

“ These are inquiries which do not belong to the present paper, as it is only intended to add some facts to those already laid before the society, and in a future communication I hope still further to increase their number.”

*Med. and Chir. Rev.*



*Observations regarding the renovation of the distinctions of youth.\**

“AMONG the various circumstances which distinguish youth from old age, three of the most remarkable are, the colour of the hair, the possession of teeth, and the clearness of vision. It is singular, that many instances are to be met with, where, after old people have experienced a failing with respect to these particulars, nature has in a manner made a fresh effort to renew the distinctions of youth.

“We shall proceed to give instances where a renovation has taken place in regard to each.

## THE HAIR.

“The colour of the hair varies much in different men, during their youth ; but when they get old, it almost uniformly becomes, first grey and afterwards white. This does not happen at the same age in every case ; for some are grey as early as twenty, or twenty-five, while others have only a few grey hairs at fifty, or even sixty years of age.

“It can hardly be doubted, that dryness, or want of moisture, is a principal cause of grey hairs, and consequently, that the custom of wearing hair-powder must bring them on sooner, than otherwise would be the case. There is reason, therefore, to believe, that keeping the roots of the hair well moistened with oily or fat substances, is the best means of keeping back what so many are inclined to consider as a defect, but which, at the same time, is not consistent with the possession of good health, or the attainment of longevity.

\* From Sinclair's code of health and longevity.

“ But the singular circumstance is this, that after an individual has got grey hairs, he suddenly or accidentally loses them; and, in their stead, hair of a different colour makes its appearance. Of this the following examples may be cited :

“ It is recorded in the transactions of the royal society,\* on the evidence of Dr. Slare, that his grandfather, whose hair, about the eightieth year of his age, had become white, grew much darker afterwards.

“ It is also reported of one Mazarella, who died at Vienna, in the 105th year of his age, that a few months before his death, he had not only several new teeth, but that his hair grown grey by age, became black, its original colour.†

“ A similar circumstance is mentioned of Susan Edmonds, of Winterbourn, Hants, who died at the age of 104, and who, five years before her death, had new hair, of a fine brown colour, which began to turn grey a few months before her death ‡

“ It is also said, that John Weeks, of New London, in Connecticut, who died at the age of 114 years, lost his grey hairs, which were renewed by hair of a dark colour.§

\* Vol. xxviii.

† *Esson* on human longevity, p. 147.

‡ Ibid, p. 168.

§ Ibid, p. 286.

## THE TEETH.

“There is no particular, in respect of which former generations seem to have enjoyed a greater superiority over the present, than with regard to the duration of their teeth. A place of interment was lately opened at Scone, near Perth, in Scotland, which had remained untouched for above 200 years, and yet to the astonishment of every one, among a great number of skeletons, which were there discovered, there was hardly any of them whose teeth were not entire and sound.\* This must be ascribed to greater simplicity of diet; to the teeth being less injured by fumes from a disordered stomach; to the custom of drinking hot liquors being then unusual; and perhaps to the absence of scorbutic complaints.

“The means of preserving the teeth will be the subject of future discussion. On the present occasion it is only necessary to observe, that many examples may be quoted, where persons, having lost their teeth a second time, have got a third set of teeth, in some cases partly, in others wholly, supplying the places of those they have lost. This circumstance merits to be particularly attended to, for as Bacon has well observed, new teeth put forth in our older years, betoken long life.

“One of the first instances of this circumstance at all authentically recorded, is the case of the old Countess of Desmond, which was accounted to be so remarkable, that many consider-

\* “This curious circumstance has been certified to me in a letter from the Rev. Mr. Aitkin, minister of Scone near Perth.

ed it to be a fable. Lord Bacon himself seems to consider it as doubtful. He says, '*They tell a tale* of the old Countess of Desmond, that she did twice or thrice cast her old teeth, and that others came in their room.' But the fact is sufficiently authenticated for one of such great antiquity, and is corroborated by many other instances.

"In the Philosophical Transactions, vol. xxxviii, it is affirmed by Dr. Stare, that his grandfather, who was a native of Bedfordshire, had all his teeth strong and firm at the age of 80, and that, within five years afterwards, he had a new set. He adds, that he remained in good health and strength to the 100th year of his age, and even then died in consequence of fulness of blood. These singular events, the doctor attributes to the frequent use of sugar of which his relation was a great eater.

"It is singular that the teeth should, in this particular instance, be preserved so long, notwithstanding the use of sugar, since the ruin of the teeth is so often attributed to that article.\*

"In the Philosophical Transactions also, two other instances are mentioned, one of Joseph Shute, a clergyman, who got a new tooth when he was 81 years of age; and another, Maria Hart, who got two new teeth at 75 years of age.†

\* The Negroes have fine teeth, though they use much sugar.

† In Easton on Longevity, there are many instances quoted of a renewal of teeth, as that of Philip Laroque, p. 104; Marion Gibson, p. 225, &c. &c. There is also a remarkable instance of one in Hufeland, vol. i. p. 171.

“In the return I have received of the old people from Greenwich hospital, mention is made of one (John Moore, a native of Ireland, the oldest man in the house,) who said that he had four new fore-teeth within five years preceeding the return, one of which he had accidentally lost.\*

“I myself have seen one James Donald, an old man now living, who had got new teeth, which I had an opportunity personally of examining. They appeared to be of a much softer consistence than teeth usually are, and not fit to do the same service; and, on the whole, they can only be considered as an imperfect substitute.

“It is said by anatomists, that the foundation of three sets of teeth may frequently be traced in the jaw of man. But if that is often the case, it is surprising that instances are not more frequent of such teeth being obtained.

#### THE SIGHT.

“There is also reason to believe, that after the sight has been lost, seemingly by a decay of nature, it has again returned, not perhaps in its former perfection, but so as to be of great use.

“One of the most singular instances of the sight being renewed, is in the case of Machell Vivan, a native of Scotland, but who was settled as a clergyman in Northumberland, and lived beyond 110 years of age. A particular account of him is

\* Lowthorp's abridgement, vol. iii, p. 297.

given by a person entitled to credit, who saw him personally in the year 1657, and who declares, that his hair had become like a child's, rather flaxen; that he had three new teeth, which he, however, got with difficulty; and though, about forty years preceeding that period, he could not read the largest print without spectacles, yet, that his sight was renewed, so that no print or writing was so small that he could not read it without them. He had five children after he was eighty years of age.\*

"I am assured, from respectable authority, that the following circumstance may also be depended upon. A lady, in the county of Fife, North-Britain, who died at the age of 89, after having been under the necessity of using spectacles for several years, recovered her sight, so that for some time before she died she could read very small print, and sew linen without glasses.

"Dr. Rush also mentions an old, man (Adam Riffle, of Pennsylvania,) who, about the 68th year of his age, gradually lost his sight, and continued entirely blind for the space of twelve years, at the end of which period his sight returned, without making use of any means for the purpose, and without any visible change in the appearance of the eyes. It is singular, that after recovering his sight, he saw as well as ever he did. During both the gradual loss and recovery of his sight he was no ways affected by sickness, but, on the contrary, enjoyed his usual health.†

\* See Fuller's Worthies of England, fol. edit. 1662, county of Northumberland, p. 309.

† "Medical Inquiries and Observations, by Benjamin Rush, M. D. printed at Philadelphia, anno 1793, p. 312."

"Several other instances of a similar nature may be quoted,\* but these are sufficient to establish the general principle, that aged people may have this distinction of youth renewed.

"It is singular, that no particular instance has occurred of the sense of hearing being renewed, after being lost by a decay of nature, or the effects of old age. It is to be observed, however, that the human race are not so apt to lose their hearing as their sight. In the return from Greenwich hospital of 96 old men beyond 80, the organ of vision was impaired in about one half, whereas the organ of hearing only to the extent of about a fifth. But this circumstance can easily be accounted for, as the eye is certainly a more delicate organ than the ear, and more liable to a variety of accidents."

*Med. and Chir. Rev.*

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*On the Blood of Persons affected with Jaundice.*

M. DEYEUX, a Parisian chemist, has made several experiments which seem to shew, that the yellow tinge the serum of

\* "See *Easton on Longevity*, account of Thomas Edgar, p. 195. and Jane Allen, p. 215. An intelligent physician informs me that he knew an old lady of above 70, who had used spectacles at 50, and about 70 could sew fine work without them. She had cartilaginous substances on the gums, which appeared to her as new teeth. When these changes took place, she had a regular monthly discharge of blood from an issue somewhere about the knee. She was so renovated as to walk miles."

the blood exhibits in jaundice is not owing, as commonly supposed, to the presence of proper bile in the blood-vessels. The serum in this state, has neither, he says, the odour nor the taste of bile; nor is alcohol, by being digested on it, impregnated with any degree of bitterness. It is remarkable, that the crassamentum, in the cases examined by M. Deyeux, was not reddened in the usual manner by exposure to air; nor did the serum coagulate by the application of heat.

*Med. and Chir. Rev.* •



*Reece, on the good effect of Acetate of Lead, in Hooping Cough.*

SIR,

I AM not aware that the acetate of lead has been ever employed or suggested as a remedy for the hooping cough: if it have, you will of course suppress this communication. The disease appearing in my own family with unusual violence, and resisting the common remedies of emetic and nauseating doses of ipecacuan and tartarized antimony, full doses of cicuta, assafoetida, oil of amber, &c. &c. I resolved on giving the acetate of lead a trial, in consequence of having found it a most valuable medicine in abating the cough and quieting the hectic symptoms attendant on phthisis pulmonalis. I commenced by giving to a child of four years old a tea-spoonful of the following mixture every six hours:



℞ Plumbi acetati	gr. v.
Syr. Violæ	ʒij.
Aq. Rosæ	ʒij.

The cough being on the following day less frequent and violent, and the stomach or bowels not deranged by the medicine, I directed the dose to be increased to two teaspoonfuls every six hours. After the first dose the child was not heard to hoop, and after two days more the cough entirely ceased. The child's general health, which for some time had been bad was evidently improved by it.

The result of this experiment induced me to give it to my youngest child, and several others in the neighbourhood, in which it proved so successful, that I really considered it a specific in those cases, and therefore am desirous to recommend it to the attention of practitioners, through the medium of your very valuable and justly esteemed Review. I have also found this preparation to succeed in a violent acute spasmodic affection, and in a case of mania hysterica in a young lady of eighteen years of age. I attribute its salutary effects in these cases to its power in diminishing excitability.

*Med. and Chir. Rev.*

---

*Adjunct Professor of Surgery.*

The Trustees of the University of Pennsylvania have lately added an adjunct professor to the surgical chair. Dr. John S. Dorsey was elected to this situation.

PROSPECTUS  
OF  
A NEW PERIODICAL PUBLICATION.

*By DR. TOBIAS WATKINS, of Baltimore.*

AT first view it may be thought by many, superfluous to attempt the establishment of a publication so similar in its nature to that of the *Philadelphia Medical Museum*; but when it is considered, that however wide the circulation of that valuable work, or however well stored with original matter, there must be still many important facts in Medicine which cannot find room for admission, and which may thus remain unknown to the world, the Editor conceives that his attempt, however humble, will neither be considered useless or unnecessary. No country in the world affords a more ample field for the speculations of the Physician or Philosopher than the United States of America, and perhaps no civilized country has less reason to boast of its discoveries or improvements in either science: That this is owing to any deficiency of talents in the United States, will hardly be asserted by the most prejudiced foreigner; to what then can it be ascribed but to a reprehensible paucity of proper channels, through which the observations and remarks of intelligent men may be conveyed to the world? There are as many good books withheld from public notice, through diffidence, as there are bad ones ushered into it through vanity; and those very men who would shrink with horror at the idea of exposing themselves in the characters of authors, would readily impart their store of information in the form of epistolary communication. Under such impressions, the Editor conceives it unnecessary, to offer any apology for his endeavour to augment the catalogue of his Bookseller. It would be equally unnecessary, and indeed presumptuous, to

make any promises of what the work may be, as its success and value must depend wholly upon the number and respectability of its contributors and patrons. Preference shall always be given to original communications of merit over every other, and of them the Editor must reserve to himself the liberty of selecting such only as he shall deem most worthy of publication, without feeling obliged to reply to the cavils of any correspondent who may think himself undeservedly neglected.

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## CONDITIONS.

I. The title of this publication shall be "*The Baltimore Medical and Physical Recorder.*"

II. It shall be published on a good paper and new type.

III. It shall be published in numbers, one every three months or nearly so ; so as to form a volume every year—each number shall contain at least eighty pages.

IV. The price to subscribers shall be *fifty cents* for each number, or *two dollars* the volume.

V. It shall be put to press as soon as a sufficient number of subscribers can be obtained to defray the expenses. Distant subscribers to pay one volume in advance, or obtain some friend in town to become answerable.

Communications and subscriptions addressed to the Editor *post paid*, will be thankfully received.

*Baltimore Oct. 31st, 1807*

*Information respecting the Influenza, in a Letter to the Editor from  
DR. SPALDING, dated Portsmouth, Feb. 24, 1807.*

“THE influenza made its appearance in this town about the middle of last month, and I think more than one half of the inhabitants already have felt the effects thereof.

“I think it slighter than I have heretofore seen it : very few call for medical aid. The symptoms are, a feverish habit, pain in the back, in the head, in and over the eyeballs, sore throat and hoarse cough, coryza and ozena, inflamed eyes.”

---

It will be observed that in the close of the last number of the Museum, the Editor adverted to the improper conduct of the editors of the London Medical and Physical Journal, in frequently introducing into their work, copious extracts from the Museum, without any reference to the source from which they are taken. The Editor would have rested satisfied with the remarks then made, had he not lately received the number of the Medical and Physical Journal for October last, in which out of 32 communications given, only the moderate proportion of 18, are taken from the Museum, occupying 48, of a number consisting of 96 pages. Of this number three only, are said to be taken from the Museum; six are merely noticed as addressed to Dr. Cox, of Philadelphia; and eight have not even this partial acknowledgement from whence taken; but appear to be introduced in the manner they are, with a view, to possess the numerous readers of that work, with an idea of an immediate correspondence with the authors of the essays. How far this conduct is consistent with propriety and decorum, the Editor leaves to the judgment of the public; observing only, that such illiberal plagiarism, does not speak greatly in favour of that extensive correspondence which they generally notice in their annual address to the public.

The Editor however is determined to lay claim to his own right, and strip these Daws of their borrowed plumes, whenever he shall perceive that due justice is not rendered him. He trusts however, a more proper sense of literary justice will in future govern them, in duly noticing the source of their extracts, if they should perceive any thing in the Museum worthy of their selection.

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# I N D E X

TO THE

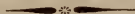
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